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Subject: Cold Fusion News No. 22

To: rlg2@yktvmv

X-Vms-To: MINT::"rlg2@yktvmv"

Dear E632 and WA84 Colleagues,

CERN, 13 - 17 April 1990.

COLD FUSION NEWS No. 22

FIRST ANNUAL COLD FUSION CONFERENCE.

Summary

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LEP Progress

SUMMARY

The two main items are the second paper from Pons and Fleischmann and the First Annual Cold Fusion Conference which was the most unusual meeting I have attended. Here I will give the overall impression of the Conference. On return from a Collaboration meeting, hope to have time to give further and more detailed comments.

On 23rd March, the first anniversary of the announcement, there was a meeting

of the Supervisory Board of the National Cold Fusion Institute, NCFI. By chance I attended the last part of the meeting which was open once you got past the locked doors of the NCFI. Reports were given of the last quarter's activities. Great attention was paid to a statement by Haven Bergeson that there may be a hint of something, though he did not want to say too much as it was outside the normal range of the counter - later I saw this very preliminary peak which was inconsistent with the other data and which normally one would check first. There were some gentle questions and then the State of Utah's board approved continuation of funding(see below), though money will run out soon and there have been no offers of funding apart from a gift from an anonymous donor (was told who he is - a hero in some societies but not in other societies).

A major topic is the experiment of Mike Salamon et al. who did not observe any neutrons or gammas when positioned below four of Pons's cells. They have new strong evidence against any fusion product emission at a time when Prof. Pons said one of his cells gave heat. This was published by Nature during the conference. Now there are strong rumours of a law suit against Salamon, Bergeson and the other authors - do hope this is not correct as a law court is not the best place to settle a scientific argument - though it may be good for lawyers.

Obtained more precise information about what is happening in Japan and India.

Thought that there was now no cold fusion experiments being performed in Europe, but have now heard of three labs in Southern Europe performing experiments.

1. IMPRESSIONS OF THE FIRST ANNUAL COLD FUSION CONFERENCE

The conference was quite unlike any scientific meeting I have ever attended because in addition to the attempt to be scientific, there was a strong fervour that seemed almost religious among most of the people present. The use of the words "Believers" for the good guys and "Skeptics" for the others, is indicative. The first talk was by Stan Pons and was firstly a rebuttal of some of the criticisms and secondly a very mathematical description of their calorimetry (I have been trying to urge Martin Fleischmann to do a simpler experiment with a constant temperature bath and a closed calorimeter which would not require such complicated corrections - no luck). At the end of Stan's talk a paper was distributed and will be discussed. The final talk was by Martin and was not technical but was very successfully inspirational for, something I had not seen before at a scientific meeting, most of the audience got to their feet applauding strongly and longly. I naturally applauded, for I like Martin - estimated about 80 to 90% of the audience were on their feet but it was difficult to estimate precisely as I was sitting down.

All of the talks were by people who are Believers, except for Nate Hoffman who said he was neither a Sceptic nor a Believer. The result was that the 10% of the world's data that was positive, was fully presented, but the other 90% that was negative, was not presented. Both at the meeting and at a local TV discussion plus phone-in show, I protested that at a Scientific meeting ALL the data must be presented and discussed. This did not happen but at least it was admitted by Believers that most of the results were negative.

The meeting had about 230 people signed up of whom perhaps ten to twenty were sceptics and of whom three (Richard Petrasso, Steve Kellogg and myself asked questions several times (note at the start of each session the Chairman read out a formula saying that audio or video recording was not allowed - though the organisers made one - and questions should only be addressed to the matter that had been presented - at one time this caused the Chairman to try and cut me off when I commented on something the speaker had said in answering a question - though he apologised privately later). It was quite possible for sceptics to attend but none were asked to speak except one who refused nor were any asked to be members of the two panel discussions.

At a Scientific meeting am accustomed to there being review talks and to there being summing-up talks - i.e. to give some perspective. Here there

was no serious attempt to summarise the subject or theory. It was a series of isolated talks. No one took all the positive results and asked whether they were self-consistent or not. Rather one counted the number of positive results. It was said you could get more positive results if you did certain things, but no one took all the positive results and asked if this were true. The nearest thing to a review talk was by David Worledge of the Electrical Power Research Institute, EPRI, who seemed to accept the positive tritium results but to have reservations about the others. He considered the problem of reproducibility to be dominant and said the EPRI would not fund major studies until the reproducibility problem was solved but would fund many short experiments. I agreed with many of his comments. Up to now I have not seen a single good clean convincing experiment giving excess heat and nuclear products at a reasonable rate and at a correlated time. I had hoped that we would hear the results of the series of experiments with 32 cells that Stan Pons started in January at the NCFI, but no results were available.

There have been many theories of Cold Fusion. Believers said that their problems were (1) how to explain the non-reproducibility, (2) the low rate of nuclear products compared with the heat claimed and (3) the ratio of tritium to neutrons said to be a hundred million instead of the ratio of one from theory and from experiment. However before the conference I was told that a famous Italian physicist would explain all these problems. This he did (reference to an earlier paper is *Il Nuovo Cimento* 101(1989)845) and was one of the dominating characters of the Conference. Prof Guilano Preparata of Milan is indeed well-known for his unusual theories - have since been told by several people that he has explained the gravitational wave results of Weber that are in disagreement with other results and theory by many orders of magnitude and also he has explained the results of a French scientist who found that water has some remarkable properties - the more you dilute it the stronger its effects become - and who was exposed by Nature and contradicted by commissions of enquiry.

There were about 100 media people there. The Press Conferences were fascinating. Some local journalists were favourable to Cold Fusion but most of the media were doubtful. A very senior official of the NCFI attacked the media which did not go down too well - it caused one journalist to say you think we are stupid but when Dr.B. says that he observed tritium production for twelve days and then shows a graph with a peak followed by a steadily falling curve, that we do not realise that it should rise continuously so that really what he observed was a high rate for a short time and then a decay for ten days a contradiction not commented on by the regular scientists in the meeting. The media expected Fleischmann and Pons to attend at least one press conference, and protested when told this would not happen. They protested even more when they learnt that there had been a private "press" conference for a very few selected journalists.

All good religions must have a Devil that you can boo. John Maddox and his journal, *Nature*, well fulfilled this role. There was another physicist whose name began with K who also elicited some reaction. However in general the tone of the meeting was friendly - in this it resembled a scientific meeting where one could disagree but still stay friends, more than a religious one. Was in the bar until midnight with some of the strongest Believers trying to play Liar's Poker - but found out we were playing different rules! The good humour was such that at the end of the meeting Richard Petrasso took the microphone with difficulty, from Martin and said that "there are exciting things that need to be explained", and said that in addition to the particles that Martin had listed as possible candidates to explain the results namely "Wimps, Champs, Dineutrons, Dubions and Morrisons", one should add : "Skeptions".

For the all-important question of Funding; the DOE has said they would give two million dollars next year - for cooperative experiments. However in Pathological Science, such experiments are rare - the only previous one in Cold Fusion was between Moshe Gai and Steve Jones It was a good short experiment but finished in serious disagreements. However Steve Jones announced an amazing joint experiment between BYU and U. of U. - which he said jokingly,

deserves the Nobel Peace prize! The leaders would be Steve and Hever Bergeson. Good Luck! Have not heard yet if the experiments chosen for funding by DOE will be peer-reviewed. There are two sections of EPRI that independently award funding. It is possible that the total would come to over a million dollars. As long as there is funding, research will continue. So when I am asked if there will be a Second Annual Cold Fusion meeting - I answer "Yes", though I would also expect there will be a smaller attendance by participants and by media. This is assuming there is no "smoking gun" or messy law suits.

2. PAPER BY PONS AND FLEISCHMANN

After the end of the opening talk by Dr. Pons a paper was made available. As this is only the second written document from them, it will be described and commented on.

2.1 CONTENT of PAPER

The paper was signed by Stanley Pons and Martin Fleischmann (not yet by Marvin Hawkins though it contains new experimental data). It is 25 pages long plus four figures. It is entitled "Our Calorimetric Measurements of the Pd/D system: Fact and Fiction". In a footnote is written "This paper has been accepted for publication in, and copyright has been assigned to, the Journal of Fusion Technology. It will appear in the July issue. Two other papers dealing with this subject, including the full paper corresponding to the preliminary note, have been accepted for publication in other journals and will appear shortly" - one will be in the J. Electroanal. Chem.

The abstract says "In this paper, we emphasise the technique, model and experimental procedures exactly as we have used them during the last few years in our calorimetric investigations. We have chosen to give this summary in the context of what others have said we have done and what we actually did".

The first part of the paper answers 7 criticisms and is followed by a short Discussion. Table 1 list 28 rods which gave excess power and Table 2 lists 8 Palladium and 6 Platinum rods which gave no excess power and which are called blank experiments.

The criticisms answered are (1) Recombination, (2) Gas and liquid purity determinations, (3) Mixing, (4) Control of water baths, (5) Estimate of heat transfers, (6) Heat transfers - Black Box model, (7) Blank experiments. The greatest part of the paper is mathematical analyses of heat transfers in parts (5) and (6).

In Part (5), the heat transfer from the cells to the water bath is analysed in terms of radiation and conduction, in particular when a calibration is made by adding joule heating using the electrical heater (basically this means that the cell's temperature is raised for three hours and the rate of heating and cooling are observed).

In Part (6), a "black box" model is introduced. The transient from the inhomogeneous non-linear differential equation is fitted to the experimental data. As the curvature of the hyperspaces converges badly, simplifications are used and the optimization is reduced to four parameters and this allows an error matrix to be evaluated. The resultant calculated error in the total output enthalpy is found to be about 0.1% which is in the range 0.1 to 10 mWatts.

In the Discussion, it is pointed out and Dr. Pons showed a graph, that the excess enthalpy rises as the current density increases and there is "no indication of a limit". At 1 A/cm² the excess enthalpy reaches 100 W/cm³. However at intermediate current densities (shown as 64 mA/cm²) there is a scatter of results, much greater than the experimental errors, and this is taken as a sign of a threshold phenomenon.

The above are considered "baseline" excess enthalpies. In addition "bursts" have been observed and the most prolonged of these is shown, lasting some 500 hours. The power output is 17 to 40 times the total enthalpy input to the cells. These burst and baseline outputs are 100 to 1000 times any that could be generated by a chemical process; "We fail to see how such large specific enthalpies could be attributed to anything other than a nuclear process". They say it is not clear whether these excesses are linked to the

production of tritium or neutrons though "the tritium levels increase markedly following a "burst" (factors of eight have been observed in the NCFI laboratories) but these increases are insignificant compared to the heat produced, if we assume the "normal" tritium output channel is responsible" - by more than ten orders of magnitude actually .

The position on Blank experiments is unusual "Our view has always been that a palladium electrode that does not show excess enthalpy in D2O is the most appropriate blank". However the paper gives more conventional controls in that six experiments are reported with platinum rods and no excess heat was found. Five experiments are reported with palladium electrodes and normal LiOH instead of LiOD as the electrolyte (could not find any statement that H2O was used instead of D2O, but this is a reasonable assumption) and no excess heat was observed.

The paper concludes "We note that the use of energy efficient systems would give energy producing systems even for some of the baseline excess enthalpies already produced."

In the question time afterwards, Dr. Pons told Richard Petrasso that gamma ray measurements would be discussed by Dr. Fleishmann, but this did not happen. In a reply Dr. Pons said that they believed that volume effects were dominant but that there was strong evidence from other experiments that surface effects are also important.

2.2 COMMENTS

It is interesting to analyse the results given in Table 1. In the original 23 March 1989 press conference, the results were expressed as the ratio of the power out to the power in and it was said that for one Watt in four Watts out were obtained. A week later this ratio rose to ten. These were apparently "baseline", i.e. steady power production and gave great encouragement to the dream of "inexhaustible source of energy". Hence in Table 1 we would expect excess heat in the range 300% to 900%, but in fact 27 of the 28 have less than 70%, and of these 22 have less than 30% and 9 less than 10%. Thus all except one cell has less than 70% excess power.

Looking at the one cell that has an excess enthalpy of more than 70%, it has 112%. This sounds encouraging. Now a frequent criticism of people who fail to observe excess heat is that their current density was too low. As mentioned above, 64 mA/cm² is considered a threshold. Now the one cell with a value of 112% had a current density of 8 mA/cm².

In considering the above numbers it should be recalled that the error in the total output enthalpy is claimed to be 0.1%.

In Table 1 errors from the regression analysis are given to between 0.1% to 5%. However no errors are given for the experimental values though it should be possible by studying the spread of measurements during the long period of time each cell was run for (there is said to be a three month mean measurement cycle time).

In the text it is written "The increase in the excess enthalpy with current density is very marked and at least of the order I squared". However it is instructive to ask how the percentage excess varies with current density. For current densities of 8, 64, 128, 256, 512, and 1024 mA/cm², the average excess heat values are 68%, 17%, 25%, 10%, 23% and 40%. In view of the very wide spread of results given above it is difficult to draw any clear conclusion other than to say that the authors could claim roughly comparable percentage excess heat at all current densities. Thus the excess enthalpy would increase as the current density to the power one of the current and not at least two as claimed.

In the paper the "allegations in Nature that we had not carried out blank experiments" are refuted by noting that there was reported one blank experiment with a sheet electrode. However in Table 2 it can be seen that the current density was 0.8 mA/cm² which has elsewhere been considered to be too small.

3. NEW RESULTS FROM THE EXPERIMENT OF SALAMON ET AL. IN PONS'S LAB

It will be recalled (Cold

Fusion News No 18, 30 July) that Mike Salamon et al. set up their apparatus to measure neutrons, gammas, electrons and protons below a table in Pons's lab on top of which were four cells. This paper has just been published in Nature of 29 March (i.e by coincidence during the Cold Fusion Conference), vol 344(1990)401. They report upper limits between a picoWatt(E-12) and a microWatt(E-6) during the five weeks of running in May and June 1989. This was even though a cell was observed to boil for two hours (they were told by Dr. Pons that they should not "reference these events as being due to release of excess thermal energy"). The Salt Lake Tribune reported Dr. Pons as saying "We are not at all surprised by their results" as the cells they were monitoring were running at barely detectable levels. "We have purposely kept the power amounts low on these cells" explaining that he and colleague Martin Fleischmann are trying to "lower our error bars" in their heat detection. With the second Pons and Fleischmann paper, it is now possible to estimate numerically what these "barely detectable limits" are. They are 0.1 to 10 milliwatts. These values are many orders of magnitude greater than the upper limits of Salamon et al. and hence it must be concluded that the barely detectable heat that Dr. Pons was observing is not of nuclear origin. i.e. not fusion.

A new controversy has arisen. Dr. Pons informed the authors that during the period when their apparatus was off during a power failure, "there was a two-hour segment in which there was an excessive thermal release from cell 2-1". At first the authors thought they could not have detected such an event as the power was off, however K. Dexler told them that neutrons would activate the ^{23}Na of their sodium iodide detector to produce ^{24}Na and this decays with the very convenient half-life of 15 hours! Re-analysing their data they were able to give upper limits of 10 mWatts(E-2) for the d(d,p)t reaction and a microWatt(E-6) for the $\text{d(d,n)}^3\text{He}$ reaction. This would seem to be even stronger evidence that whatever was causing Dr. Pons to claim excess heat, is not fusion.

Since the Fleischmann-Pons claim of excess heat is said by the authors not to be Chemistry and has been shown by Salamon et al. not to be Nuclear, there appears to be only one explanation.

4. NATIONAL LABORATORIES

The comment has often been made that National Laboratories have not observed Cold Fusion. Hence there was relief for Believers when scientists from Los Alamos and Oak Ridge presented positive results from their labs. Indeed one very strong Believer sent me a Fax asking my opinion of the work of Scott et al. at Oak Ridge reporting the existence of excess heat, neutron emission, gamma ray activity and tritium formation. Below is a summary of my reply;

a) Tritium; in my notes is written "within errors canNOT say any tritium" and in their conclusion they did not claim any tritium.

b) Neutrons; Their biggest effect is $21 \pm 3 \frac{1}{2}$ over a period of four hours. This is only one bin. It is a rate of 0.0015 neutrons/second which is about E-15 Watts or a femtoWatt. Later there was a result suggesting an excess rate of 30 neutrons per day or $\frac{1}{3}$ E-15 Watts or $\frac{1}{3}$ femtoWatt. Again this rate is so low

it seems doubtful for a single counter. There seems to have been only one counter whereas experience has shown many times that a single ^3He or BF_3 counter often gives spurious signals. Thus a single counter is not trustworthy.

c) Gammas; They find an excess in the bin 2.64 to 3.14. Where does this come from? - mostly likely from radioactivity, especially Thorium. I missed hearing any numerical estimate of the rate e.g. in Watts

d) Excess Heat; A variation of the power in the low range of joules/second was shown - missed hearing any numerical estimate of Watts or of the ratio of power out to power in. This is the only result worth further study - would be pleased to have details of calibration, temperature control etc.

Overall there seem to be major inconsistencies of many orders of magnitude in the power outputs. The experiment is a small one, surprisingly so for so large a laboratory.

Both Oak Ridge and Los Alamos seem to have been doing minor experiments and

not making full use of the facilities available. The only US National lab that seems to have made a substantial effort on Cold Fusion is Sandia - and they found no effects.

In other Western countries some national labs have made major investigations - at Harwell in the UK, at Karlsruhe in West Germany, at the Paul Scherrer Institute in Switzerland, at Mol in Belgium. All found nothing. The biggest Cold Fusion experiment in the world was at Harwell where six million dollars worth of apparatus was used and it cost half-a-million dollars. Here they did not use one neutron counter but 56 counters so that any erratic behaviour by one could be excluded by the other 55.

5 JAPAN

Since the earliest days, the threat that Japan might develop an American discovery, has been brandished. At the CF Conference, Dr. Ikegami of the National Institute for Fusion Science told me that the Institute was established for Hot Fusion research. It has six large divisions working on different types of Hot Fusion. There is also a section for special projects of which Dr. Ikegami is the leader. When the Utah results were announced, his section allocated 2% of its budget of \$5 million to Cold Fusion - that is \$ 0.1 million. In February there was a meeting of Japanese interested in Cold Fusion and some thirty papers were presented. Of these about ten were theoretical, ten found no effect and ten found positive effects. These were essentially universities or similar institutions. There are known to be a number of private organisations working on Cold Fusion, but the scale of their activities and their results have not been made available.

6. INDIA

The Bhabha Atomic Research Centre, BARC, has been working extensively on Cold Fusion. The Director, Dr. P. K. Iyengar, gave a very interesting introductory talk. He showed an impressive photograph of BARC and said 13 000 people worked there of whom 3000 were scientists. He said they had never budgeted for this type of research, so 140 volunteers have done the work but no money has officially been spent. A publication, BARC 1500 describes the work of six groups during April to September 1989.

Dr. M. Srinivasan presented the results in detail and gave a generally excellent impression of competence. The results of the six groups were that neutrons were produced in bursts but in small quantities, and that tritium was produced abundantly, the ratio n/t being about $E-8$. However this was based on six values, namely $E-8$, $E-9$, $E-7$, $E-8$, $E-3$ and $1.4 E-6$ which to me suggests a very wide spread and the average is less than $E-8$. Steve Kellogg said that if the tritium came from the reaction $d + d \rightarrow t + p$, then the tritium should once in 5000 times give the reaction $t + d \rightarrow {}^4\text{He} + n$ and this neutron would have 14 MeV which would be easily recognisable. Dr. Srinivasan said they had looked for energetic neutrons but had not found them. Since BARC 1500, some 6 or 7 experiments have been performed. On March 15th tried a Pd/Ti cell and said "if do not get neutrons on first day will pack up" - got neutrons on the first day. This seems to me to be a dangerous technique, almost as bad as a professor telling a research student that he would not get a doctorate if he did not find a certain result.

Although there were earlier reports of work on Cold Fusion elsewhere in India, there were no reports of work other than at BARC.

7. SOLAR NEUTRINOS

In the last Cold Fusion News, the importance of a possible disagreement between the solar neutrino flux measured and theoretical expectations, was shown. The present best experiment to measure this is the Kamiokande detector in Japan. However it was proposed to shut down this detector to install a Cold Fusion cell. Explained this to Steve Jones who was not aware of this and of the possible importance of the present maximum of the sun spot cycle. However have just had a message that the Kamiokande experiment is continuing measurements and results will be presented at the important bi-annual

Neutrino conference to be held in CERN in June.

8. OPPENHEIMER-PHILLIPS MECHANISM

People have been seeking for a mechanism to increase the probability of deuterons penetrating the potential barrier, as normal calculations show that there is the enormous factor of ten to the power -40. One hope was that the Oppenheimer-Phillips mechanism might help. This was because the Coulomb field acts only on the proton of the deuteron and not on the deuteron centre of mass. Thus the deuteron could be polarised and the hope was that the neutron would lead into the palladium nucleus reducing the barrier factor. This has now been calculated by S.E. Koonin and M. Mukerjee (Caltech report MAP-129) who find the effect is negligible, changing the rate by less than 1%.

9. SELLING A PATENT

Many people have taken out patents, and one who has a good track record of successful patents, is reported to be on the point of selling it for a million dollars. However the money has to be put into escrow and if the system works as advertised, the money will be paid.

Douglas R. O. Morrison.

CORRECTIONS

The article "Rise and Decline of Cold Fusion", CERN report CERN/EP 90-36, seemed to be appreciated as restoring some balance at the conference, however some corrections to it and to News No.21 were given to me.

1. Japan does not have a National Cold Fusion Research Institute - as explained above there is a Fusion Institute which was intended for Hot Fusion research.
2. On the 24 March 1989, Steve Jones did not have a press conference - the announcement was made by BYU - Steve was told he could be absent.
3. The fun particle invented by Edward Teller which he called the Meshugtron from the word "meshuga" - this word is from Yiddish and not Hebrew.

LEP PROGRESS

LEP and the four LEP experiments are all running well with the normal amount of interruptions. A record beam accumulation of 3.1 mA has been obtained (but not for physics). The low beta squeeze has been reduced from 7 to 4.3 cm and this was found to give a 50% increase in rates in the experiments. At times they are obtaining 10 Z0 events per minute. Each experiment has about 15 thousand new Z0 events this year.

When Cold Fusion Got Hot, It Rapidly Fizzled

Science: The promise of unlimited energy set off a frenzy of activity. But publicity alone couldn't make claims of a breakthrough true.

By STEVEN E. KOONIN
and NATHAN S. LEWIS

"Cold fusion" was born a year ago today when chemists Martin Fleischmann and B. Stanley Pons at the University of Utah held a press conference to announce that they had tamed the process of nuclear fusion.

Their claim was novel and the apparatus simple—a rod of palladium, a battery and a jar of water. The promise of unlimited energy set off a frenzy of activity and publicity as researchers everywhere rushed to confirm the Utah results.

"Scientists would have been ecstatic if cold fusion had been real. But publicity alone didn't make it true. Confirmation by independent researchers was the only way to be sure. Regrettably, the present consensus is that Pons and Fleischmann were simply wrong. This follows not from prejudice, but from many careful experiments performed and thoroughly documented by interdisciplinary teams using apparatus far more sophisticated than that available to the Utah researchers. The continuing claims of a few "believers" are plagued by irreproducibility, inconsistencies and the absence of peer-reviewed publication. Most telling, however, is that nobody else can make cold fusion work.

The research community responded as it should have. Chemists, physicists and materials scientists dropped work in progress and focused on the fusion claims. Results emerged—with surprising speed—from universities, national laboratories and industry. While it is disappointing that this effort came to naught, validation is an integral part of the scientific process. Any phenomenon that doesn't fit our understanding of the physical world stimulates a scientist's curiosity. High-temperature superconductivity is an example where the process had a more favorable outcome.

Cold fusion underscores the interdisciplinary nature of today's science. The concept intertwines chemistry, materials science, condensed-matter physics and nuclear physics. Complete knowledge of all these fields is simply too much for any one scientist to master. This doesn't diminish the role of individual initiative and creativity, but today's science requires cooperation among experts in the various disciplines. Indeed, scientists racing to test the Utah claims immediately formed teams to pool their talents and experience. If Pons

and Fleischmann would have done so in their early experiments, cold fusion might have expired quietly.

Instantaneous communication among scientists is now possible through the fax machines and computer networks that link laboratories around the globe. These tools hastened cold fusion research, but the giant game of "telephone" also corrupted information and spread rumors. It accounted for the speed with which the scientific community moved to test the idea, as well as for much of the confusion in the days after the Utah announcement.

Manipulation of the popular media was instrumental in creating and sustaining the furor. Spectacular claims, such as the observation of nuclear reactions, were publicized without sufficient details to allow informed judgment by other scientists. Reports of heat production greatly exaggerated the tiny effects that researchers had actually measured. Reporters who sought independent assessments were hampered by scientists' natural hesitation to judge the work of others without knowing the details. As the facts became fully known, all of the highly touted claims were withdrawn or severely qualified. The usual procedure of peer review and full publication in a scientific journal is circumvented only at one's peril.

Pressure for funding distorted the scientific process but couldn't derail it. The Utah Legislature committed \$5 million to a National Cold Fusion Institute on the assurances of a panel made up largely of businessmen, lawyers and publicists. To its credit, the federal government sought better scientific advice. An expert group commissioned by the Department of Energy visited all of the laboratories claiming to have observed cold fusion. Its members didn't see a single "working" device, even in the lab of Fleischmann and Pons. Qualified peer review remains the best method to allocate our precious and limited research funding.

Modern science is a most exciting endeavor. While progress occurs at an astounding rate, most advances are not widely publicized. Similarly, the errors that do (and must) occur are quietly corrected through the scientific process. True progress withstands the test of time. Although cold fusion excited our imagination, in the end it was just another corrected mistake. Thus, the lessons it teaches are more important than the experiments themselves. We scientists, the media and the curious public would do well to remember them when trumpets herald the next unverified discovery.

Steven E. Koonin is a professor of theoretical physics and Nathan S. Lewis is an associate professor of chemistry at Caltech.

cellor Helmut Kohl has repeatedly affirmed West German opposition to a "special German path." Kohl and Minister Hans-Dietrich Genscher stand very clearly the core elements of Germany's success in the past and future—a process of change toward open, prosperous and free Europe in the nation in the center, Germany, tightly to its neighbors in a dense network of economic, political, social and cultural linkages. The chancellor has stated and over again that the history of the 20th Century "shows that nothing is detrimental to the stability of Europe as a Germany swaying between two worlds between West and East."

Kohl is right. The basic fact remains due to history and geography, Germany's world will always be complicated by the necessity of balancing seemingly conflicting interests and by the possibility of rekindling resentment and fear. We must measure to its efforts to stand with its neighbors rather than stand aloof.

To be neutral is to be aloof. A neutral must prove constantly the

GERMANY LBO of the FRG

Greenmail

■ If Michael Milken pop into this dream, they'd new wall to keep the We Germans out.

By FRANKLIN E. ZIMRING

Here was my dream after pizza and reading the newspaper at night:

The negotiations between the Germanys take an unexpected turn when Michael Milken, the junk-bond maven, flees to the Democratic Republic and offers his expertise in exchange for safe haven. Next week, the East German government will announce a leveraged buyout offer for the Federal Republic of Germany.

Since the East German currency is suspect, the GDR offers each German man, woman and child in West Germany marks in the form of bonds to be issued by the new government and perhaps some tax advantages. There is early talk of a Munich and some NATO based on gender cash flows, but that turn of events is unnecessary—generations of deficit governance have left the Germans so cash-rich that the

29 MAR 90 10: 81

-R.L. GARWIN-

FRIDAY, MARCH 23, 1990
THE WASHINGTON POST

Fusion Confusion Offers Window on Utah's Psyche

Ambitious, Insecure State Lunged at Respect in a Beaker

By Rick Atkinson
Washington Post Staff Writer

SALT LAKE CITY—Exactly one year after a newspaper headline here proclaimed, "Fusion Discovery Could Rank as Century's Greatest Achievement," life in Utah has gone a bit flat. There has been no Nobel Prize, no economic boom, no Sodium Chloride Valley to rival California's Silicon Valley. Yet, ever hopeful, a squad of scientists keeps searching for further signs of nuclear fusion in their experimental flasks, which resemble nothing so much as the proverbial watched pot that never quite boils.

Last March 23, this state seemed to have the universe by the tail. Two chemists announced at the University of Utah that they had sustained nuclear fusion—the power that makes the sun burn—in a beaker of water at room temperature. Astonishing in its simplicity, the experiment was hailed in a university news release as having the "potential to provide an inexhaustible source of energy."

Having commanded the world's attention, many Utahans embraced "cold fusion" with unbridled boosterism only to see the giddy zeal succeeded by disappointment and even embarrassed defensiveness when other scientists failed to confirm the breakthrough. Today the scientific phenomenon remains a mystery—intriguing, inexplicable and apparently no closer to commercial exploitation than it ever was.

In fact, the fusion confusion may tell more about the psychological structure of Utah than the nuclear structure of the stars. Cold fusion played to both the ambitions and insecurities of a beautiful state with many quality-of-life amenities that, nevertheless, seems to worry a great deal about its image. "Everybody wants recognition, to be loved and respected," said Thane Robson, a prominent economist at the University of Utah. "But if you live in a remote and isolated region, maybe you desire those things somewhat more."

"And in the West there's a strike-it-rich mentality. The big bonanza is right up the creek," Robson adds. "If you don't understand that mentality, you can't understand why the university president would hold a press conference with such hoopla to announce this fusion discovery. It may not play well in the Ivy League, but, friend, it plays well in this region and in this state."

When the claims failed to pass out, Utah became the butt of jokes that took a swipe at the state's scientists and its Mormon asceticism. "They say they have cold fusion in Utah," comedian Mark Russell quipped. "You can't even get a cold beer in Utah." Of such derision, Utah political scientist J.D. Williams said simply, "It hurts. It hurts. It hurts."

"A lot of realism has settled in over the last year," Robson said dryly.

Last spring, realism was not an overabundant commodity here. Before March 23, the cold fusion experiment had been so secret that university officials referred to it among themselves as "the F-Project." Once the discovery was announced, however, Utahans accustomed to chatting about ski conditions and basketball suddenly found themselves palavering over palladium lattices and plasma physics—and the potential transformation of this arid swatch of the West into a scientific mecca. The state legislature met in special session to pump \$5 million into the fusion venture.

The university bookstore sold "Utah Fusion Busters" jay chains and T-shirts; a Mexican eatery brewed a fruit drink called



Martin Fleischmann, left, and Stanley Pons at news conference last year defending their research.

the Cold Fusion and sold 55 in two hours. Local songwriters cranked out "Cool Fusion Blues," "I've Been Playing With Palladium" and "Do the Fusion," which included these lyrics: "All you critics make a lot of hoopla/About crazy ideas from the state of Utah/Just remember when you're puttin' us down/We invented TV and stereo sound."

Among the animating anxieties here was a fear that someone else would snatch away the "crazy idea" of cold fusion. "We were being advised by our patent attorneys to establish primacy," James Brophy, vice president for research at the University of Utah, said of the news conference last March. "In my thinking, if this thing had the possibility of being the \$100 billion activity that it could be, would we have the right to risk losing it for Utah? I think not."

Utah could use \$100 billion. Much of the state economy rests on mining, timber, agriculture and energy industries, each of which had a troubled decade in the 1980s. For six consecutive years, wages, when adjusted for inflation, have dropped and more people have moved out of Utah than have moved in.

"Everybody was hoping that cold fusion was the beginning of a new Silicon Valley," said Williams, a ruddy, red-haired professor who came to the University of Utah in 1952. "We sit here as if in a drying-up oasis, waiting for a water-bearer to come over the horizon with something that will save our economy."

A similar phenomenon occurred in 1962 with the development here of the Jarvik artificial heart, a bold-medical experiment that generated a great hubbub but never lived up to its promise. (Two months ago, the Food and Drug Administration recalled the device, ruling that risks outweighed benefits.) In the case of cold fusion, when scientists elsewhere last fall appeared to breathe some hope back into the Utah experiments conducted by chemists B. Stanley Pons and Martin Fleischmann, an editorial in the *Deseret News* pounced on the news: "The vindication of Pons and Fleisch-

mann is a vindication of Utah and the University of Utah." But soon after, a Department of Energy panel concluded that the Utah researchers' results failed to warrant special federal funding.

"It's kind of an emotional thing with the people of Utah," said Hal Fox, president of the Fusion Information Center, a smeltinghouse dedicated to spreading the gospel of cold fusion. "After all, we're kind of a little peanut state out here in the wil of the West, and this is a big thing. . . . We like to pat ourselves on the back."

The "yearning for a winner and for making a contribution to the common good," another observer put it, in turn reflects the dominant Mormon culture in a state where 70 percent of the citizenry belong to the Church of Jesus Christ of Latter Day Saints. "Here's a religion," said economist Robson "that believes it has a worldwide mission to fulfill, a religious mission to save the world. Cold fusion, by promising to solve Earth's energy needs, fit the bill nicely."

Today, the university's research effort are consolidated in the National Cold Fusion Institute, quartered in an unobtrusive warren of offices and laboratories once occupied by a pathologists' association. Financed largely by the state, the institute opened last summer and has about 20 researchers and five administrators pursuing variations of the Pons-Fleischmann experiment.

The university will sponsor a cold fusion conference next week in an effort to sift through the conflicting experimental data reported by chemists and physicists around the world. Although some have reported symptoms of a nuclear reaction—so-called fusion in a flask—the results have been frustratingly hard to replicate consistently, which is crucial to commercial exploitation.

But hope clearly springs eternal. "A wife won't let me say 'sex,'" said Fox, "so I say cold fusion is the most important discovery since Adam's rib."

Staff researcher Lucy Shackelford contributed to this report.

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Cold fusion

Received: by YALEVM (Mailer R2.03B) id 8975; Sun, 08 Apr 90 16:47:42 EST
 Date: Sun, 08 Apr 90 16:47:27 EST
 From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
 Subject: F.Y.I.
 To: Dick Garwin <rlg2@watson>

===== 67
 Received: from CERN.cern.ch by YaleVM.YCC.Yale.Edu (Mailer R2.03B) with BSMT
 id 7542; Sun, 08 Apr 90 13:04:23 EST
 Received: from CERN by CERN.cern.ch (Mailer R2.06) with BSMT id 3545; Sun,
 08 Apr 90 19:03:02 GVA
 Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
 Sun, 08 Apr 90 19:03:00 GVA
 Received: by dxmint.cern.ch (cernvax) (5.57/3.14)
 id AA12873; Sun, 8 Apr 90 19:00:40 +0200
 Message-Id: <9004081700.AA12873@dxmint.cern.ch>
 Date: Sun, 8 Apr 90 19:03 GMT +1
 From: MORRISON%VXPRIX.decnet.CERN@cernvax
 Subject: Cold Fusion.
 To: gai@yalevm
 X-Vms-To: MINT::"gai@yalevm"

Dear Moshe,

8 April 1990.

Thanks for your messages. I had hoped to meet Kelvin Lynn at the Cold Fusion Conference - too bad as sceptics were very few - essentially Petrasso, Kellogg and myself. John Huizenga was there but had decided not to speak in public (though he did so in private). However the Press was happy to present a balanced view - I even got a paragraph in the Deseret News which must be a first.

The conference was really quite remarkable. Am starting to write it up so more then. As I arrived on the 22nd March was in contact early and was told of a wonderful theory by prof. Preparata. I tried to warn them that he was rather special. When I told my colleagues at CERN that a well-known Italian physicist had produced a theory that explained all Cold Fusion results, they all guessed "Preparata". One told me he had had a theory of water with memory. also have just heard that he has a theory to explain Weber's unusual results on the observation of gravitational waves. Showed the theory to a good theoretician who did not turn up at Preparata's talk as he said the theory was so bad that it just was not worth while, e.g. the energy involved was so great that the palladium rod should explode. He attended Schwinger's talk and criticised it. The CF believers seemed to prefer Preparata to Schwinger, perhaps because he was more aggressive.

There were two people from GE. Philip Kosky has good political judgement. The other, R. Wilson, criticised the theory of F&P at the end of Pons's talk. This was reprinted in the NYT. He was then in trouble with his GE management as GE has an agreement with F&P. Just before Fleischmann's closing speech, Wilson gave an apology, saying he had been misled by equation 3 but now he had seen the full paper, he had realised that eqn. 18 explained everything and was correct. Preparata then seized the microphone and demanded that Wilson write to the NYT withdrawing and apologising. Wilson said he had tried to see the reporter but he had left. Preparata again insisted he write and apologise. Philip Kosky invited me to GE to give my lecture on "N-Rays, Cold Fusion and Pathological Science that I have given a few times. It did not seem possible but as I was worried about Wilson, I tried to arrange something, but with flight restrictions, it was not possible. If you hear anything, please let me know, as I would not like an innocent person to suffer. Maybe if Gary Taubes has a smoking gun, this might help in time, otherwise with funding from

DOE and EPRI, it is quite possible that there may be a Second Annual Cold Fusion Conference.

You were wondering why I was praising Steve Koonin - it was for his Los Angeles Times article. I told the Salt Lake Tribune about it and they reprinted it.

Best Wishes,

Douglas.

PS Gave out a large number of copies of my article "Rise and Decline of Cold Fusion" to participants at the conference and to the media - who said they were helped to obtain balance. So today was delighted to find at an Antique Fair a book that I have been wanting for a long time - Gibbon's "Decline and Fall". The 18 volumes were published between 1788 and 1794 so it must be close to a first French edition. Since the French Revolution occurred during this period, the last volume is dated both 1794 and year 3 of the revolution.

MORRISON cernvax 4/08/90

MORRISON%VXPRIX.dec gai@yalevm

4/08/90 Cold Fusion.

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7 MAR 90 10.74

-R.L. GARWIN-

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BRIGHAM YOUNG
UNIVERSITYPhysics Department
296 Eyring Science Center
Provo, UT 84602 U.S.A.
Telefax: (801) 378-5474
Telephone: (801) 378-4361From: Steven JonesTo: Richard GarwinTelefax: (914) 945-4419 Department: _____

Address: _____

New YorkTelephone: (914) 945-2555 Date: 3/7/90

New Energy Times Archive

✓
Cold
fusion
logfile

BRIGHAM YOUNG
UNIVERSITYTHE GLORY OF GOD
IS INTELLIGENCE

March 6, 1990

Dr. David Lindley
Nature Magazine
1137 National Press Bldg.
529 - 14th Street
Washington, D.C. 20045

Dear David,

I am re-sending herewith a memo sent to you in Nov. 1989, which presents the factual evidence that deuterium did indeed enter the Ti 662 chips. It is exasperating to have our Nature paper rejected in part because "There is in your work no direct evidence (a pressure drop in the cylinder, for example) that D₂ was taken up by any of the metal samples." (Lindley to Menlove, Feb. 20, 1990). The pressure drop in the cylinders is discussed with numbers shown direct from Mike Pacciotti's log notes in the attached letter which was sent to Moshe, etc., and you in Nov. 1990. We measured both acoustic emissions and pressure drops and should add this to the Menlove paper.


You raise two other questions to which I will respond briefly here. 1) The H₂-control cylinders contained Ti chips and were temperature-cycled. The Menlove paper states clearly: "Hydrogen gas control experiments with four SAMPLES loaded with H₂ gas in place of D₂ gas" (p. 10 of Rev. 3, capitals added). 2) We distinguish between background events and "source" events in several ways discussed in the paper (see esp. p. 4 of Rev. 3): the neutron bursts predominantly occur at a sample temperature of about -30°C; we split the signal output to 128μs and 16 μs gates and required that the ratio of counts be consistent with neutron die-away time in the detector; and we note that background events were all less than 30 source neutrons, whereas source events register up to 300 neutrons (Figures 1,2). (Recently an event of about 400 source neutrons was detected by Menlove, in two segments of a counter each having its own electronics.)

Finally, let me say that a follow-up experiment at LANL is now being mounted. Ironically, phone calls to the director of LAMPF opposing this experiment resulted in a committee review which lead to more support of the experiment than we would otherwise have had. In particular, we now have access to the LAMPF electronics

DEPARTMENT OF PHYSICS AND ASTRONOMY
296 EYRING SCIENCE CENTER
BRIGHAM YOUNG UNIVERSITY
PROVO, UTAH 84602
(801) 378-4361

pool. We will do the experiments with open log books as usual and invite observers -- I propose Richard Garwin. Perhaps then we will be allowed to publish "without being damned."

Sincerely,



Steven E. Jones

cc: Richard Garwin, Kelvin Lynn, possibly others.

New Energy Times Archive

Nov. 14, 1989

FAX to:

Moshe Gai and co-workers 203 432 3522

Kelvin Lynn and Jim Reilly 516 282 4071

John Hack 203 432 2797

Kurt Zilm 203 432 6144

Dear colleagues,

A few quick notes:

→ In Menlove's "Update on the Measurement of Neutron Emission from Ti Samples in Pressurized D₂ Gas" which I sent you,* it is stated that Ti pieces that do not crack under temperature cycling (as evidenced by a paucity of acoustical emissions) have not produced bursts. One strength of the Los Alamos effort is the acoustical pickup on the sample cylinder. Cracking of the oxide layer on temperature cycling presumably opens fresh surfaces to permit permeation of deuterium. Thus, it is not surprising that we often have to wait for several cycles (over several days) before neutrons are seen at Los Alamos (see Update). The fact that some deuteriding does occur is indicated by increased brittleness of Ti chips following weeks of exposure to deuterium, and a decrease in the deuterium gas pressure. Surface preparation is probably very important to reproducibility, and we have tried variations in partially removing the oxide layer but with no particular consistency as yet. A set of instructions used successfully by Mike Paciotti of LANL and myself is enclosed to show ^{one} variation~~s~~ used. _{tuo}

The greatest consistency so far is that bursts predominately occur at around -30°C (Menlove submission to Nature and Update). This remarkable observation provides hope for controlling and understanding the phenomenon. In discussions with John Hack this summer, I learned that Ti alloys have the property that hydrogen in solution enters the hydride phase with a maximum rate at approximately this same temperature! (Graph attached.) When the hydride forms, the material swells by about 18%, then, since the hydride is brittle, cracking is common. A paper by Pardee and Paton (Metal. Trans. A, 11A:1391-1400, 1980) describes the effect and its temperature-dependence. This phenomenon does not require much hydrogen in the metal since "hydrogen tends to diffuse toward and concentrate at a region of large tensile stress" (Pardee p. 1391, enclosed) -- the attached graph shows the strong peaking in hydride/crack growth at -30°C for Ti 6%(Al) 4%(V) for only about 250 ppm hydrogen. This is one of the Ti alloys used successfully in the Menlove experiments. The Pardee paper identifies several parameters which affect hydride growth -- "temperature, hydrogen concentration, time, magnitude of the stress, and solubility of hydrogen in the alloy" -- so that it may take some time to identify and control critical parameters. People like John Hack and Jim Reilly are clearly needed. Meanwhile, it certainly appears difficult to assign neutron bursts at -30°C to systematics, but the correlation with optimum hydride/crack growth is remarkable, don't you think?

I can also report that Professor Yutaka Maeda of Kyoto University, Japan, reported on Nov. 4 in a question/answer period the observation of (infrequent) neutron bursts in Ti samples in pressurized D₂ gas, at the RIKEN (Tokyo, Japan) Conference "Foundation and Applications of μ CF Phenomena" where I also discussed cold fusion. This was an open meeting. He uses helium-3 counters and sees bursts of about 200 neutrons or so ^{on warming from LN₂ temp}.

I will send word on the analysis here in about two weeks. (Of course, we do not expect to finish everything that soon.)

*Enclosed is the Menlove Update in case you haven't seen it.

Sincerely,

Steven E. Jones

11/14/89

Steven E. Jones

M. Pacioth

2 Jun 89

Preparation for Ti 6-6-2

(Menlove experiment pd-1)

- 1) Lathe turnings ^{from BYU} with cutting fluid (unknown material)
- 2) Broken up to smaller pieces (small enough for tank entry)
 Condition of the control sample (still have oil)
- 3) Cleaning inside the tank (once we realized that
 the cutting oil had been used)

May

both can
be straw-
ration

- Methylene chloride flushes (5-6 flushes)
 — no ultrasound —
- methanol flushes (5-6)
- Pure water flushes (5-6)

- 4) Pumping & drying with cryopumps at $\approx 50^\circ\text{C}$
- 6) Clean (molecular sieve trap) H_2 flushes to 1000 psi
- 7) Clean H_2 flushes at up to 220°C
 (30 min at 220°C)

← Note

- 8) Cool and fill to 425 psi H_2 at room temp
- 9) add D_2 to 850 psi (all thru trap)
- 10) 20 May first cooling LN_2 cycle

21 May another LN_2 cycle ← Bursts at room temp

25 May another LN_2 cycle ← ?

- 11) 1 Jun 89 ~~the~~ pressure has dropped by $1\% \pm 0.3\%$. Material looks the same
 size to us (no solution into smaller pieces)

M. Paciotti - Notes on preparing alloys
for pressurized D_2 cylinders

1 Jun 89

Load 2ea) 500 cc tanks

<u>D_2</u>	<u>$D_2 + T_2$</u> (← not used)	<u>Material</u>
10.2 g	8.8 g	Ti 662 from original batch that went into successful experiment pd-1 — unwashed — has same smell
20.7 g	20.0 g	Ti 6-4 (6AL 4V) machined dry.
48.4 g	48.7 g	Ti 6-4 (6AL-4V) machined with black cutting oil. Washed with
79.3 g	77.5 g	trichlorethane, then methanol

- 1) The Ti 662 was broken up from long
curled turnings
- 2) The Ti 6-4 was done on mill from a ~~the~~
rectangular stock. The chips were more
heavily curled and ~~some~~ much more
fractured on the side away from the tool.
Tool was Carbide with Ti coating.
- 3) All the chips are very brittle and break
with a snap.

Expts DD2 & DD3 (500 cc tanks)

5 Jun 89

Contents of each tank:

10g Ti 66-2 from Steve J. (unwashed)

20g Ti 6-4 machined dry

48g Ti 6-4 machined in dark oil, washed in trichlorethane and then methanol

- 1) Wash inside tank with methylene chloride, then methanol, then water
- 2) Pump with sorbion pump at about 60°C to dry
- 3) Let up to N_2 & replumb
- 4) Pump again to 5μ at pumps then longer lines
- 5) Pressurize to 1060 D_2 at room temp
heat while bleeding down pressure (160°C at 500psi)
- 6) Go back to 500psi (190°C at end of ramp)
- 7) While bleeding out gas, hydrating starts in one tank.
(sucked up about 1 mole at least)
- 8) Suspected a leak so kept above atmos with low pressure D_2
(added even more D_2)
- 9) $> 320^{\circ}\text{C}$ in one tank^{DD3}, cut heat ($\approx 210^{\circ}\text{C}$ in other tank^{DD2})
- 10) Cool to room temp & raise to 750 psi & down to 30psi
- 11) Back to 853.4 psi and valve off hydrated one. (DD3)
- 12) The DD2 tank begins to react at 850 psi — up to 43°C —
Cool it & drop to 200 psi ~~Do not~~ not using much at 200 psi
- 13) Try to stabilize it at 100 psi — still falling
used 200 + 20 + 20 psi in 500 cc

 $\approx 0.33 \text{ mols } \text{D}_2$

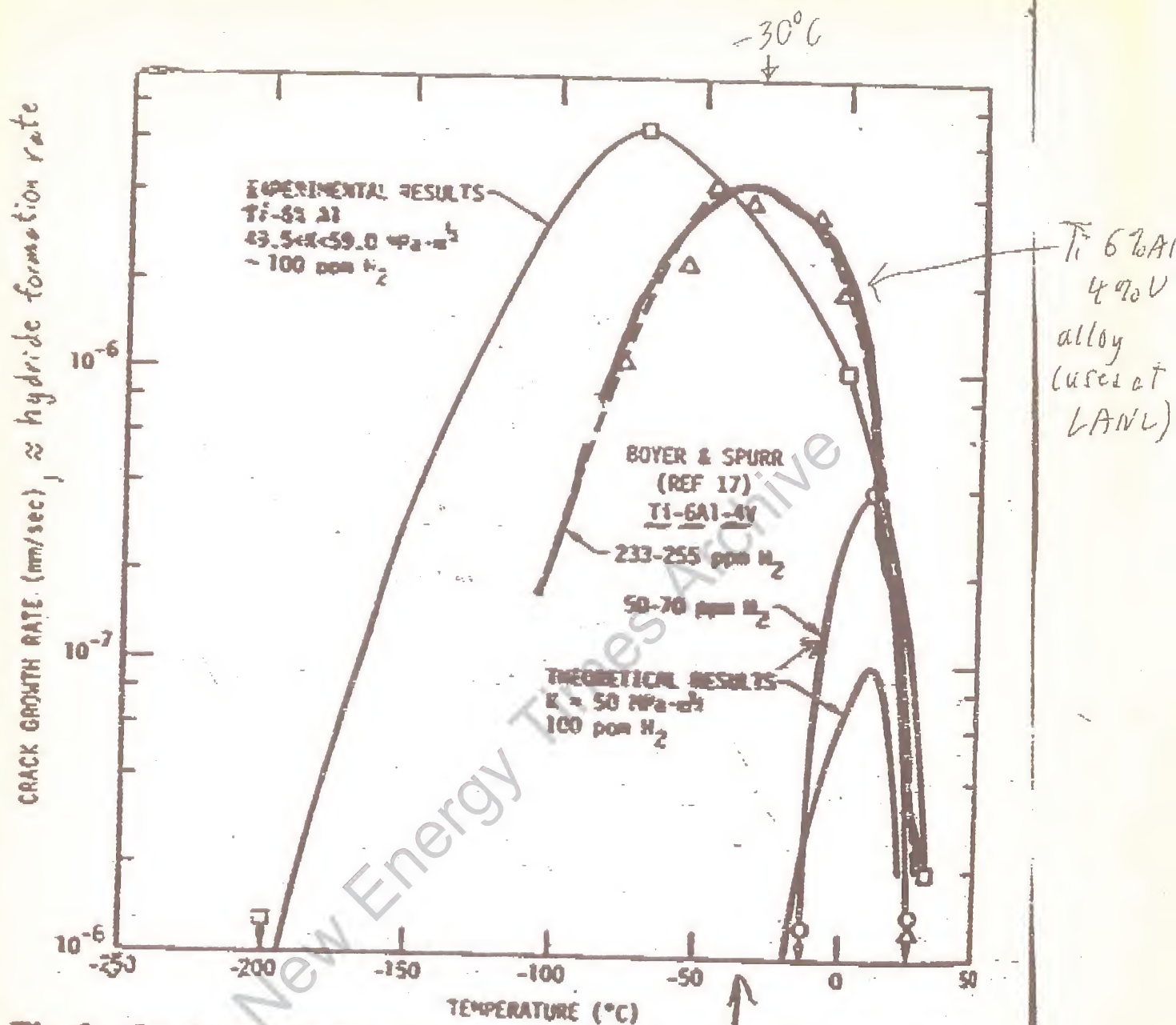
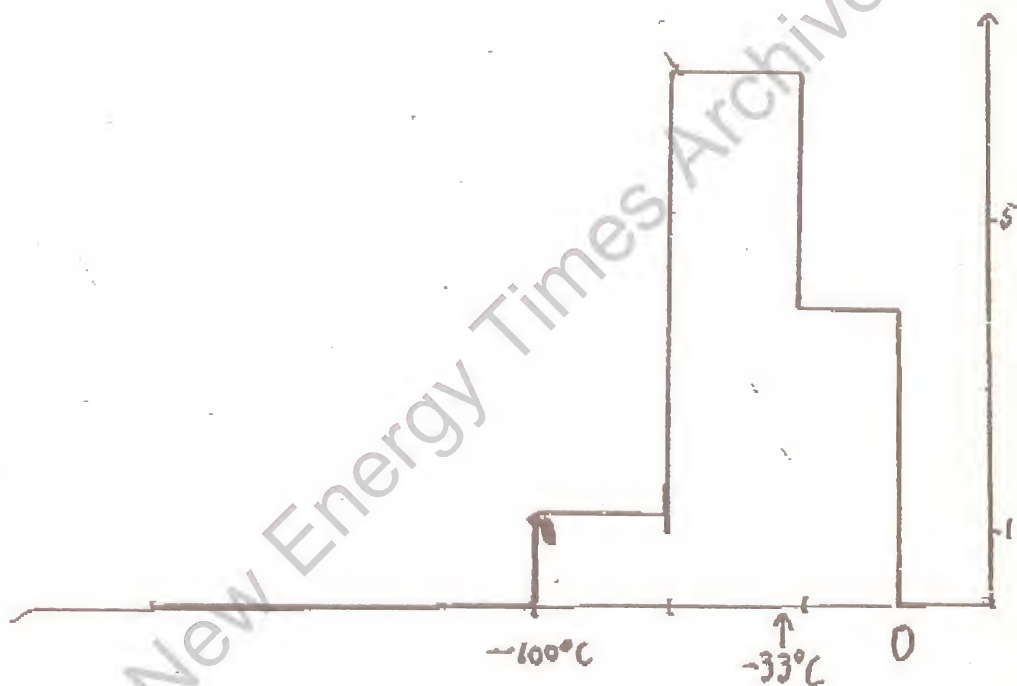


Fig. 5—Crack growth rate plotted vs temperature for Ti-6 pct Al and Ti-6Al-4V (Ref. 17). Experimental and theoretical results have similar functional behavior, but the theoretical peak is smaller and occurs at higher temperature.

W. J. Pardee and N. E. Paton, Metal. Trans. A, 11A (1980) 1391.

neutron burst frequency
vs. temperature - LANL/BYU



*Cold fusion
by gill*

Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 9458; Fri,
02 Mar 90 19:07:21 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Fri, 02 Mar 90 19:07:14 GVA
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id AA03172; Fri, 2 Mar 90 19:03:35 +0100
Message-Id: <9003021803.AA03172@dxmint.cern.ch>
Date: Fri, 2 Mar 90 19:09 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear Dick,

2 March 1990.

To bring you up to date, am sending you a copy of a message to Tom Droege. Recently he has stopped sending mail apart to say he enjoyed meeting you briefly - am worried that because he will speak at the CF conference, he does not want to disturb things. How did your meeting with him go?

The programme of the CF meeting seems to be all positive results. Have asked Martin how negative results will be presented. See that Julian Schwinger is down to speak.

Last week we had a talk by Vic Teplitz on the START negotiations - it was very interesting and nicely presented. It was organised by Jack and John Ellis. At dinner afterwards Vic said he would like to come to the Moriond conference next week, so hope he can make it.

Best Wishes,

Douglas.

Dear Tom,

2 March 1990.

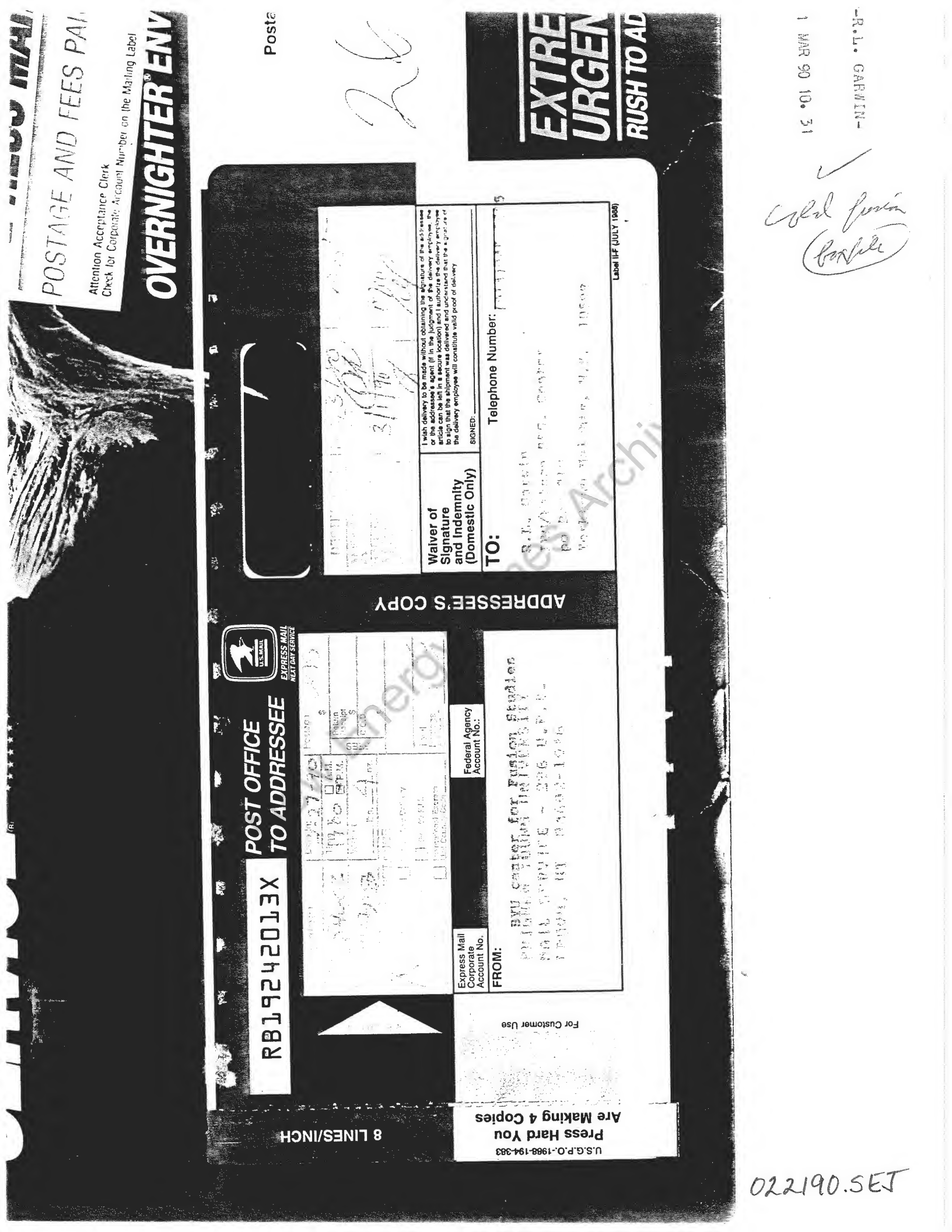
Have just received the programme of the First Annual Cold Fusion Conference and see you (and a member of your family) are down to speak. Think you will find it a very interesting experience. However it is very important that the fact you are down to speak should in no way affect your critical faculties, for experimenters must always be looking for ways to check their results and for possible sources of error.

Have just had a phone call from Garry Taubes who wrote the book "Nobel Dreams" about Carlo's and CERN's finding the Z0. He is nearing the end of his book about Cold Fusion and wished to interview me. He was surprised I refused and asked why. I explained that every person in CERN that I had spoken to and who appeared in the book, was unhappy about how they finally appeared. You may find it interesting to ask some people from CDF what they think about the book.

Looking forward to seeing you in Salt Lake City - if you like skiing it is great at Alta - I am going a few days early to enjoy it.

Best Wishes,

Douglas.



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R.L. GARMIN

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(Profile)*

022190.5ET

1 MAR 50 10. 51

-R.L. GARNIN-

Feb. 21, 1990 (finished 2/27 after visitors left)

Dear Kelvin, Kurt, and Moshe (etc.) :

I will respond to questions from Moshe in the hopes of providing (in some cases repeating) information to help with your report. However, I feel that the questions raised by Al Anderson and myself in January 1990 really ought to be addressed by you as well. In particular, Al's questions about electronics problems and calculating efficiencies for individual ring counters should not be treated lightly. [Enclosed is the discussion on Inefficiencies from Anderson's Report.]

We are now doing Monte Carlo studies of the response of liquid-scintillation counters to 2.5 (and 14) MeV neutrons as we prepare for our experiment at LANL. We routinely simulate our neutron-detection experiments in this way, and I highly recommend this approach to you as a means of understanding the response of the Yale set-up. It is not at all clear to me that the efficiencies of the Yale detectors for 2.5 MeV neutrons have been properly evaluated. Calibration with sources is a start, but you still need to unfold the response for 2.5 MeV neutrons, suggesting the need for Monte Carlo methods. Deadtime and other inefficiencies specifically identified in Anderson's report need to be evaluated. Lowering the thresholds has introduced fuzziness in the thresholds (not good) and applying 2-dimensional cuts (pulse-height/ pulse-shape) requires a corresponding re-calculation of individual detector efficiencies. You should do this.

The runs analyzed by Al so far are listed by run number in Appendix A of his report (attached); please notice that this is the first half or so of the data. There is no selection of data to enhance the number of double hits, Moshe. Rather, there were problems reading past run 53 on the raw data tapes from Yale so Al began with this (arbitrary) data set and after looking at the data, felt it sufficient until the problems in the electronics were addressed (see his report, PLEASE). It is possible that even though the electronics has been torn down, a careful evaluation of per-detector efficiencies can be done so that quantitative statements are justified, but this remains to be demonstrated. I have asked Al if he will run through all the data now as requested by Moshe; he says he probably will after the Monte Carlo work for the LANL experiment is completed, but he feels you should do Monte Carlo work, too (and other analysis as reviewed above), to make your results meaningful. He stated in his report already that "it is unlikely that subsequent review of the entire data set will be able to change any of the conclusions," (his analysis report). Please consider his report seriously.

We are proceeding with plans to measure the time structure of neutron bursts (if any) at Los Alamos, although not at LAMPF now. Moshe requested information about the experiment. The experiment will incorporate BOTH Menlove-type helium-3 proportional counters

1

022190.5ET

Inefficiencies

The efficiency of the apparatus consists not only of the probability that a neutron will generate appropriate pulses, but that such pulses somehow get recorded as part of an event, and that the event survives any cuts applied. Generally the integrity of the electronics should be ensured prior to data taking, but since there is ample evidence that the experiment was assembled and conducted in haste, there is some grounds for concern about whether the electronics actually behaved as advertized. The following observations are pertinent.

- The lack of an electronics diagram means that one can not be sure what correlations are to be expected and are proper in the data and waste much time inferring them. Some problems cannot be solved and may not even be recognized without such information.
- Although the logbook mentions that the detector gains were adjusted, the pulse-height spectra show great differences between detectors. Amplifier saturation was a problem. Thresholds, which are of fundamental importance in any neutron measurement, also appear to vary among the detectors. Figure 5 shows raw spectra from Cf runs which illustrate these problems.
- Detector pedestals (Fig. 6) were not adjusted, and several were at or near zero, making any energy calibration suspect. This also makes it difficult to determine the electronic inefficiency of the detector, which prefers an unambiguous electronic zero.
- A small fraction of events had no latches (Fig. 7), which although probably not significant, is disquieting because such events should be logically impossible, and when the signal to noise is as low as in this experiment, any correlation between such losses and real data could be fatal.
- The TDC's (actually TAC's) and at least some of the ADC's were not cleared after each event. Thus a detector without a latch set would contain data from a previous event. This could be a problem if there are any spurious latches. Figure 2 shows these 'leftover' events, which can be identified by comparing unlatched TDC values with the most recent latched value.
- There was substantial crosstalk between TDC's, which was not entirely eliminated by requiring latches. Figures 2 and 8 also illustrate this.
- The TDC's were not matched. This was actually helpful in identifying crosstalk, but demonstrates lack of care in setting up.
- There was an enormous (about 30 times the nominal count rates) noise source in detector D1, which exactly simulated a good time of flight. Perhaps it was a pulser either purposely or accidentally left in. In any case, it was only identifiable by lack of ADC pulses.
- The design did not include any mechanism for determining the relative times of events in the two groups of detectors. Useful multiplicity data require latches for both central detectors if their corresponding ring counters are included. This puts a 500ns window on up-down events and probably explains why so few up-down events were observed when center latches were required. A complete understanding of the effect of the latches on multiplicity requires more MonteCarlo work.

Problems of this type may be expected from a new setup, but for a previously existing apparatus put together by a supposedly experienced group, they do not increase confidence that any results will be trustworthy.

An obvious source of electronic inefficiency was that a substantial fraction of events which had good times of flight failed the neutron-gamma discrimination cut because they had no pulse shape and/or no pulse height. Interestingly enough, for runs after run 30, when adjustment of thresholds, etc. apparently ceased, the inferred inefficiency for data and background runs held steady at about 50% (after neglecting the noise in D1), while the Cf run (34) showed an efficiency of 80%.

Since Cf was used to determine efficiencies, the correction to the efficiency due to deadtime depends on what the correction was for the specific Cf runs used. Without more information on what actually went into the efficiency calculations I cannot choose a deadtime correction. Based on the observed variation both in the signals and the neutron detection rates, I am inclined to be suspicious that a single value of 0.8% is claimed to apply to all. Detector U3, for instance seemed to have less than half the efficiency of some of the others.

The correction for the more stringent neutron cuts that I used should probably be about 10%.

If I attribute the above mentioned loss to deadtime in the pulse-height or pulse-shape apparatus, then it is tempting to infer that the backgrounds have a somewhat higher multiplicity (of gammas or muons) than that of the Californium (2-3 neutrons).

The probability of self veto was given as 0.6%, so the probability of veto for a burst of 100 neutrons is 45%, which could constitute a substantial inefficiency.

The probability that a burst of 20 neutrons should have a ring multiplicity of 3 or more, is from 5% to 12%, depending on the exact effect of deadtime. If a few such events occurred, they would not be expected to be detected. Bursts of 125 neutrons have probabilities of from 78% to 99% of being detected. We can conclude that there were probably no events of more than 50 neutrons in the data runs, although calculating the exact confidence levels remains to be done. The excess of double hits in the foreground over the background in Table 1, might be consistent with some smaller bursts, but it is difficult to set an upper limit on bursts of less than 50 neutrons.

I have looked at a selection of the data consisting of about 400K foreground events representing about 56 hours worth of data. The selection also includes a corresponding amount of background and some ^{252}Cf (neutron source) data. No ^{60}Co (energy calibration) data were available. This subset of the data contains the events mentioned in the interim report, and it is unlikely that subsequent review of the entire data set will be able to change any of the conclusions.

ASSUMING
 $\epsilon_i = 0.8\%$
IS CORRECT
WHICH IS
DOUBTFUL
for ring
counters

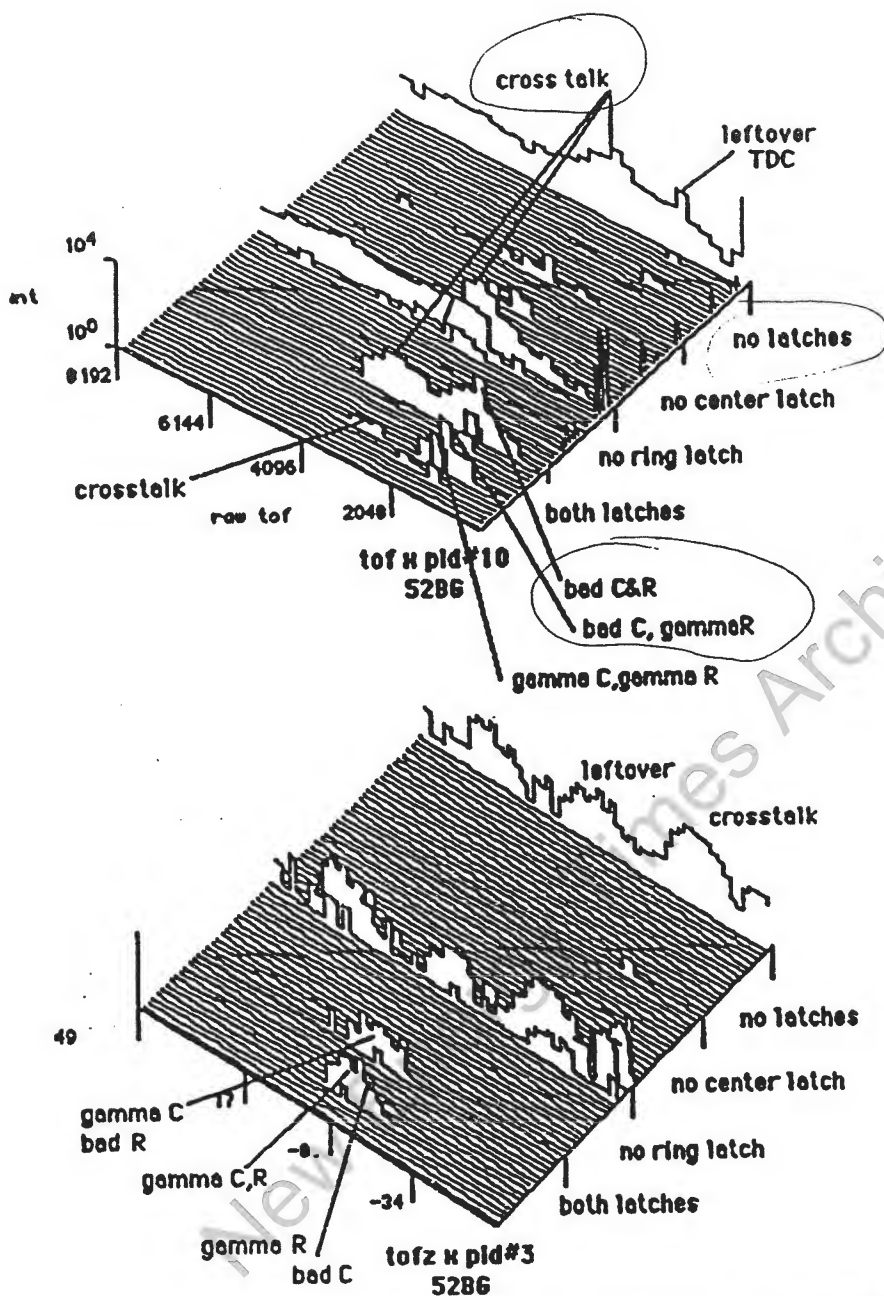


Figure 2. Particle ID vs time of flight for detectors D1 (#6) (@ 2.5ns/chan), and D5 (10) (@256ns./chan) in background run 52. Part. ID is described in the text.

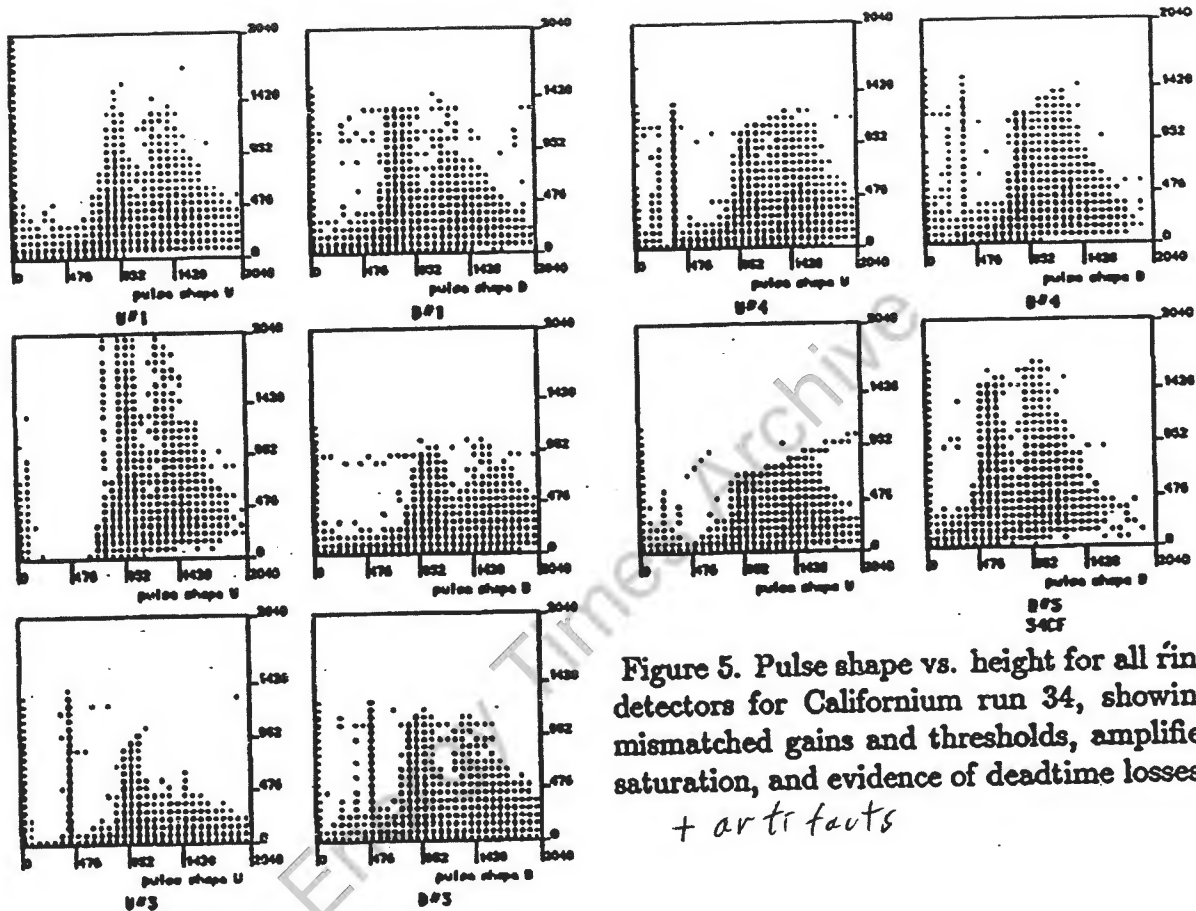


Figure 5. Pulse shape vs. height for all ring detectors for Californium run 34, showing mismatched gains and thresholds, amplifier saturation, and evidence of deadtime losses.
+ artifacts

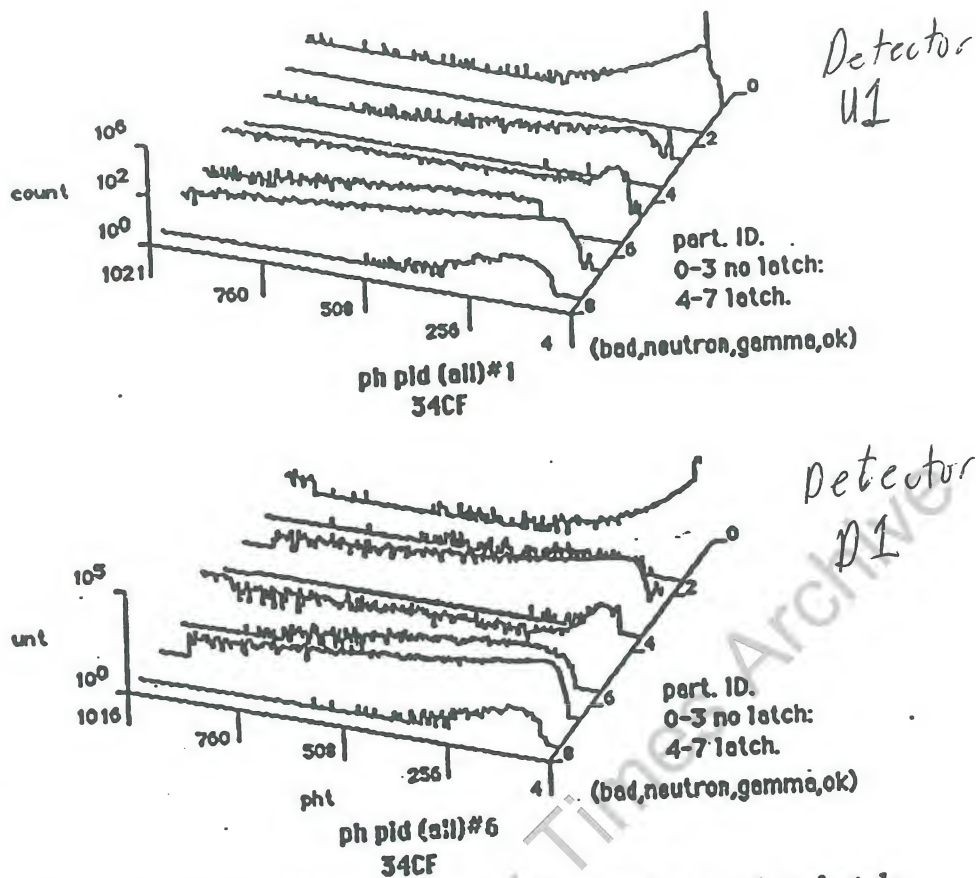


Figure 6. Pulse height vs. particle ID, showing good pedestals for detector U1 (1), and poorly adjusted pedestals for detector D1 (6). Data is from Californium run 34.

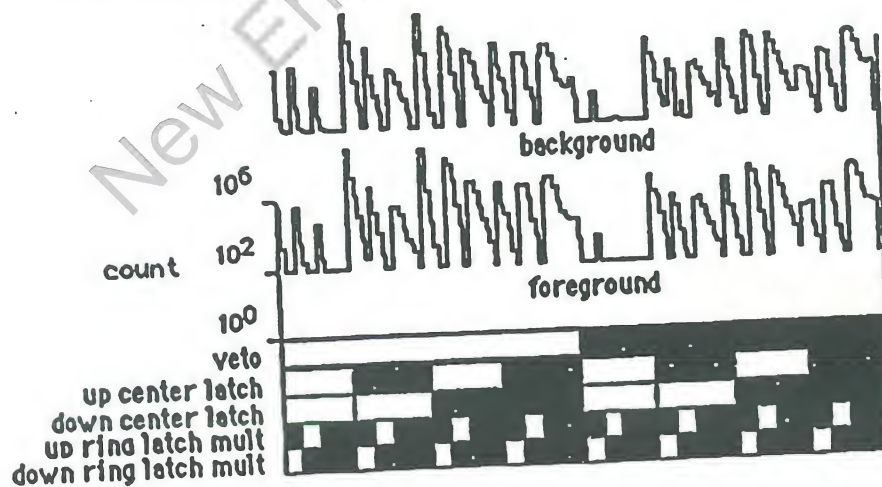


Figure 7. Latch multiplicities for all foreground and background runs. Note events with no latches.

A. Anderson, 1/90

Appendix A: Neutron counts as a fraction of total clean events.

First row after run name is total events with both ring and center latch set, with time of flight between channels 0 and 20 (relative to gamma peak), and satisfying the 'ok' event gate on pulse shape/pulse height spectra. Second row is subset which also satisfy the 'neutron' gate. Third row is ratio of previous numbers.

Run 22 etc.

tlow = 0, thigh = 20											sum	ave
run:	u1	u2	u3	u4	u5	d1	d2	d3	d4	d5		
22CF:00029	00023	00022	00025	00000	00022	00025	00029	00022	00035		00232	
(xx)00001	00000	00000	00002	00000	00000	00001	00000	00001	00003		00008	
	0.034	0.000	0.000	0.080	0.000	0.000	0.040	0.000	0.045	0.086		0.034
30BG:00007	00002	00008	00002	00002	00006	00003	00012	00001	00004		00047	
(xx)00000	00000	00000	00000	00000	00000	00000	00001	00000	00000		00001	
	0.000	0.000	0.000	0.000	0.000	0.000	0.083	0.000	0.000			0.021
31BG:00009	00020	00012	00010	00019	00020	00014	00023	00006	00019		00152	
(xx)00000	00000	00000	00000	00000	00000	00000	00000	00001	00000		00001	
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.167	0.000		0.007
32BG:00312	00332	00428	00431	00337	00327	00431	00533	00350	00487		03968	
(xx)00000	00000	00000	00001	00000	00000	00004	00003	00004	00005		00017	
	0.000	0.000	0.000	0.002	0.000	0.000	0.009	0.006	0.011	0.010		0.004
34CF:09329	08602	10304	06314	11385	13230	11462	12334	08119	16076		107155	
(CF)01590	01510	00381	01731	02505	00948	01517	02523	01641	02044		16390	
	0.170	0.176	0.037	0.274	0.220	0.072	0.132	0.205	0.202	0.127		0.153
40BG:00068	00091	00156	00026	00150	00356	00171	00233	00169	00493		01913	
(BG)00001	00000	00001	00000	00001	00002	00002	00003	00004	00010		00024	
	0.015	0.000	0.006	0.000	0.007	0.006	0.012	0.013	0.024	0.020		0.013
41BG:00001	00004	00009	00002	00006	00094	00011	00013	00004	00163		00307	
(BG)00001	00001	00000	00000	00001	00000	00000	00001	00000	00004		00008	
	1.000	0.250	0.000	0.000	0.167	0.000	0.000	0.077	0.000	0.025		0.026
424A:00002	00001	00004	00001	00002	00105	00024	00004	00002	00243		00388	
(FG)00000	00000	00001	00001	00002	00000	00002	00001	00000	00003		00010	
	0.000	0.000	0.250	1.000	1.000	0.000	0.083	0.250	0.000	0.012		0.026
434A:00135	00130	00241	00090	00325	00442	00459	00228	00338	00684		03072	
(FG)00002	00001	00000	00001	00002	00002	00002	00001	00009	00008		00028	
	0.015	0.008	0.000	0.011	0.006	0.005	0.004	0.004	0.027	0.012		0.009
444B:00075	00052	00148	00007	00220	00363	00307	00135	00129	00505		01941	
(FG)00000	00000	00001	00000	00003	00000	00005	00003	00001	00007		00020	
	0.000	0.000	0.007	0.000	0.014	0.000	0.016	0.022	0.008	0.014		0.010
454C:00098	00086	00177	00058	00249	00346	00332	00419	00264	00538		02567	
(FG)00000	00000	00000	00000	00000	00002	00001	00002	00005	00007		00017	
	0.000	0.000	0.000	0.000	0.000	0.006	0.003	0.005	0.019	0.013		0.007
464D:00067	00049	00129	00009	00204	00448	00367	00492	00215	00528		02508	
(FG)00002	00001	00000	00000	00003	00001	00000	00006	00001	00006		00020	
	0.030	0.020	0.000	0.000	0.015	0.002	0.000	0.012	0.005	0.011		0.008
474D:00012	00007	00036	00002	00019	00202	00167	00146	00041	00250		00882	
(FG)00000	00000	00000	00000	00001	00001	00000	00001	00000	00001		00004	
	0.000	0.000	0.000	0.000	0.053	0.005	0.000	0.007	0.000	0.004		0.005
52BG:00029	00076	00007	00033	00107	00152	00152	00141	00095	00226		01018	
(BG)00000	00002	00000	00000	00000	00000	00001	00001	00001	00003		00008	
	0.000	0.026	0.000	0.000	0.000	0.000	0.007	0.007	0.011	0.013		0.008
534G:00024	00065	00004	00008	00089	00151	00143	00118	00112	00206		00920	
(FG)00000	00001	00000	00000	00002	00000	00001	00001	00002	00002		00009	
	0.000	0.015	0.000	0.000	0.022	0.000	0.007	0.008	0.018	0.010		0.010

Ring detector

average n fraction: low 0.012 0.011 0.003 0.008 0.011 0.003 0.007 0.010 0.017 0.013

type	n	all	n/all
FG:	00108	012278	0.009 +- 0.0008
BG:	00040	003238	0.012 +- 0.0020
CF:	16390	107155	0.153 +- 0.0012
xx:	00748	015172	0.049 +- 0.0018

low

high

E; not same!
for ring counters

and liquid-scintillation detectors (LSD) as used routinely in our muon-catalyzed fusion experiments. The ^3He detector will provide a trigger on neutron bursts, and tell us the multiplicity of each burst. The LSD array of up to seven detectors will provide information regarding neutron energies and the time structure of the burst candidates. The LSD counters can be moved away from the source if the bursts appear to be very short in time (e.g., a blast in the LSD array will show up in pulse-heights and pulse-shapes and a discrepancy in M relative to the ^3He system) -- we will build in this flexibility. The electronics of the two detector systems will be distinct, providing confidence should equivalent numbers of neutrons be detected in both systems. We can provide more details later if any of you has an interest.

Moshe asks whether the fraction of time in the -100°C to 0°C range at LANL is 2% or 5% of the total data-taking time. Howard Menlove alerted me in December 1989 to the incorrect figure of 2% in the "Rough Draft" (clearly so labelled). The correct fraction is approximately 5%, he said. I checked this by looking at the temperature vs. warm-up time plot (Fig. 4 attached), and comparing the time in the -100°C to 0°C range -- 4000 s -- with the typical 1-day per temperature cycle used in the LANL experiments. In this way I get a fraction of (1.1 hrs in -100°C to 0°C range) / (24 h per temp. cycle) = 4.6%. I conclude that Menlove's estimate of about 5% is correct, and this number appears in the Update now. Incidentally, Howard feels that the total hours in the two experiments should be compared rather than just the warmup times, since deuterium permeates very slowly into the Ti samples (see further discussion below).

Moshe also asks about the coincidence time gate used in the LANL experiments. The answer is: 128 μs . The Menlove submission to *Nature* (which Moshe says he wishes to compare the Yale results with) states:

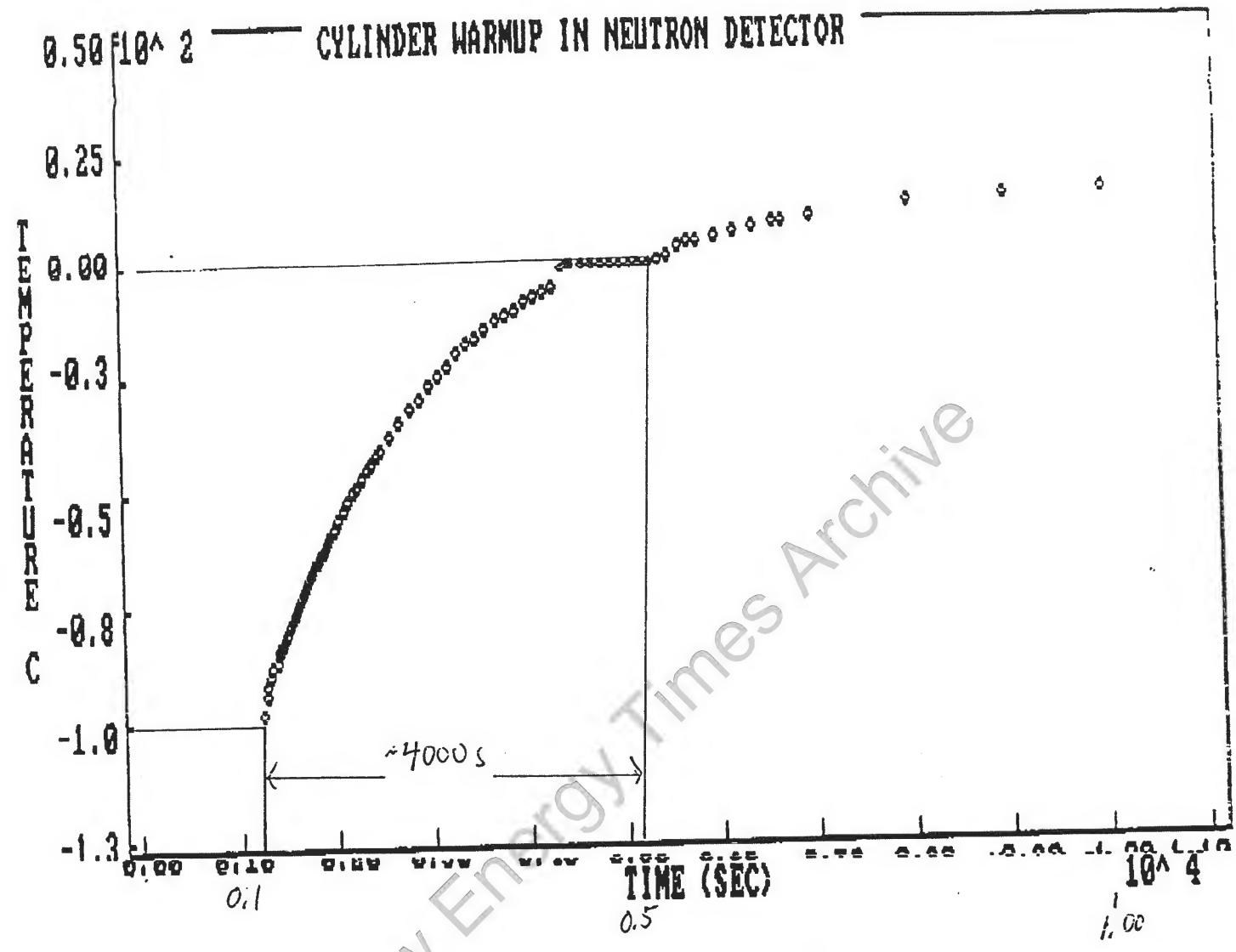
whether there are any other neutron counts within the selected time gate. We use a coincidence
 → time gate of 128 μs , which corresponds to about 3 times the neutron die-away time of the detector.

The time-correlated (coincidence) count R is related to the number of neutrons (N) that are counted in the gate by the relationship

$$R = \frac{N(N-1)}{2} .$$

Thus if 100 n are emitted from the sample in a burst of $<100 \mu\text{s}$, our System 3 would detect ~34 n and R would equal 561. For our larger bursts, we have observed N from the increment in our totals (singles) scaler, and the calculated R using the above equation has agreed with the observed R in the coincidence scaler. If the burst event lasts more than 128 μs , N will be underestimated. The accidental coincidence counts are measured by sampling the shift registers

LANL



In speaking to Howard about this, the 40-50 μ s die-away time for neutrons in the moderated system, and recent events in which R and N are compared, he concluded that the best we can say now is that the time spread in individual bursts appears to be less than 100 μ s (as opposed to 50 μ s which may have cropped up in some discussions). We will be prepared to improve on this time spread in the upcoming LANL experiments. We plan to keep the active time gate to at least 128 μ s since we do not know the time structure of burst events. In any case, the time gate over which we were sensitive to correlated neutrons (bursts) has been 128 μ s in the Menlove experiments.

You asked for detailed information regarding the LANL runs and I provided tables with burst sizes, weights, materials, loading conditions and preparation steps, etc. You find in the Update the burst sizes, and this must be known to compare the experiments. But then you are obliged to use all the information available and not select the first few weeks worth, it seems to me. (As I understand Moshe's 2/15 letter, this is what you are planning -- can this be true?) It must stretch one's definition of scientific honesty to ignore a large portion of information in the Update when you must use the Update to get needed burst sizes for your paper! Why not use an average burst rate from the information provided? Indeed, I have even calculated in detail for you average burst rates based on the Update -- should you ignore this information?

It is true that two of the early cylinders (Ti-1 and Ti-6) had an anomalously high burst count, but then so did cylinders Ti-14, Ti-16 and DD-5C, and Ti-24 taken later. (I just noticed that Ti-2 in the 6-page Table 1. I sent last time with all the info. on cylinder fills should be labeled Ti-4; correction attached.) True, one can judiciously select information in the Update I provided the collaboration in October 1989 to get large or small rates, almost at will. For instance, I find 18 bursts ($M > 10$ neutrons) in the selected period 6/20 to 7/8, which is higher than the rate in a "three week" period selected by Moshe in his recent letter ("Additional comments on your 2/2/90 letter to Garwin"). Contrariwise, I find only 2 bursts in the period 8/9 to 8/28. But this is not wise to select a sub-set of data when the average value is available and quite different.

Also, how do you know the cylinder-hours corresponding to the data sub-set you select? Perhaps you could get it from what I've sent, but I provided a carefully re-checked number for the total set provided in the Update (13,000 cylinder-hours of foreground data). If you seek an accurate comparison with Menlove, I recommend you use the average rate based on the entire Update data.

Table 1.

<u>Reaction</u>	<u>wt. (g)</u>	<u>material¹</u>	<u>loading conditions²</u>	<u>results</u>
Ti-1	100 4.2 2.1 11.0	Ti sponge Ti 6-6-2 turnings Ti crystals Ti sponge cathode	outgassed 150 °C 1 hour, backfilled 600 psi	4 bursts cycles 3,4 low level continuous emission
Ti-2	100 100	Ti sponge Ti sponge crystals	outgassed 200 °C 1 hour, backfilled 400 psi at 70 K	none
Ti-5	46.4	Ti turnings	outgassed 200 °C 1 hour, backfilled 600 psi	none
Ti-6	146.9 17.4 22.8 1.7 4.9	Ti pieces Ti powder Ti sponge cathode Pd pieces Pd powder	outgassed 200 °C 1 hour, backfilled 600 psi	8 bursts cycles 4-9 P=200 psi at end of experiment, repressurized P=580 psi, no more neutrons
Ti-7	33.1	Ti sponge predeuterided at 550 °C (TiD _{0.18})	backfilled 580 psi	none
Ti-8	50 4	Ti turnings Ti sponge	outgassed 200 °C 1 hour, backfilled 580 psi	none
Ti-2	30	Ti turnings	After high-temp. cycles	none
Ti-3	25	Ti turnings	O ₂ loaded at LN ₂ temp.	yes

Page 1 of 6 (sent before)
- corrections

It frankly appears that there is an effort afoot to get a factor of 3 or so here or there to make the Yale experiment compare more favorably with the much longer LANL experiment. It seems wiser to me to take what has been learned and do a longer, better experiment to find out what is (or isn't) happening, rather than to strain to prove a point. Accuracy is more important than "being right," but it takes time.

Please don't exaggerate, or ignore information provided you.

Now I will respond to the issue of how much deuterium we have in our test samples. We have discussed this before. Moshe says that "an oxide layer, which was not removed in the Menlove treatment, prohibits (according to all experts on material science) the formation of the deuteride." (Letter of 2/12/90.) In Moshe's 2/15 letter to me it is argued that "our material science people found that it is very unlikely that the Ti samples in your Los Alamos experiment included deuterium."

I find data more convincing than an appeal to authority. Please look again at the data which I sent to you earlier (with my 2/2/90 letter to R. Garwin):

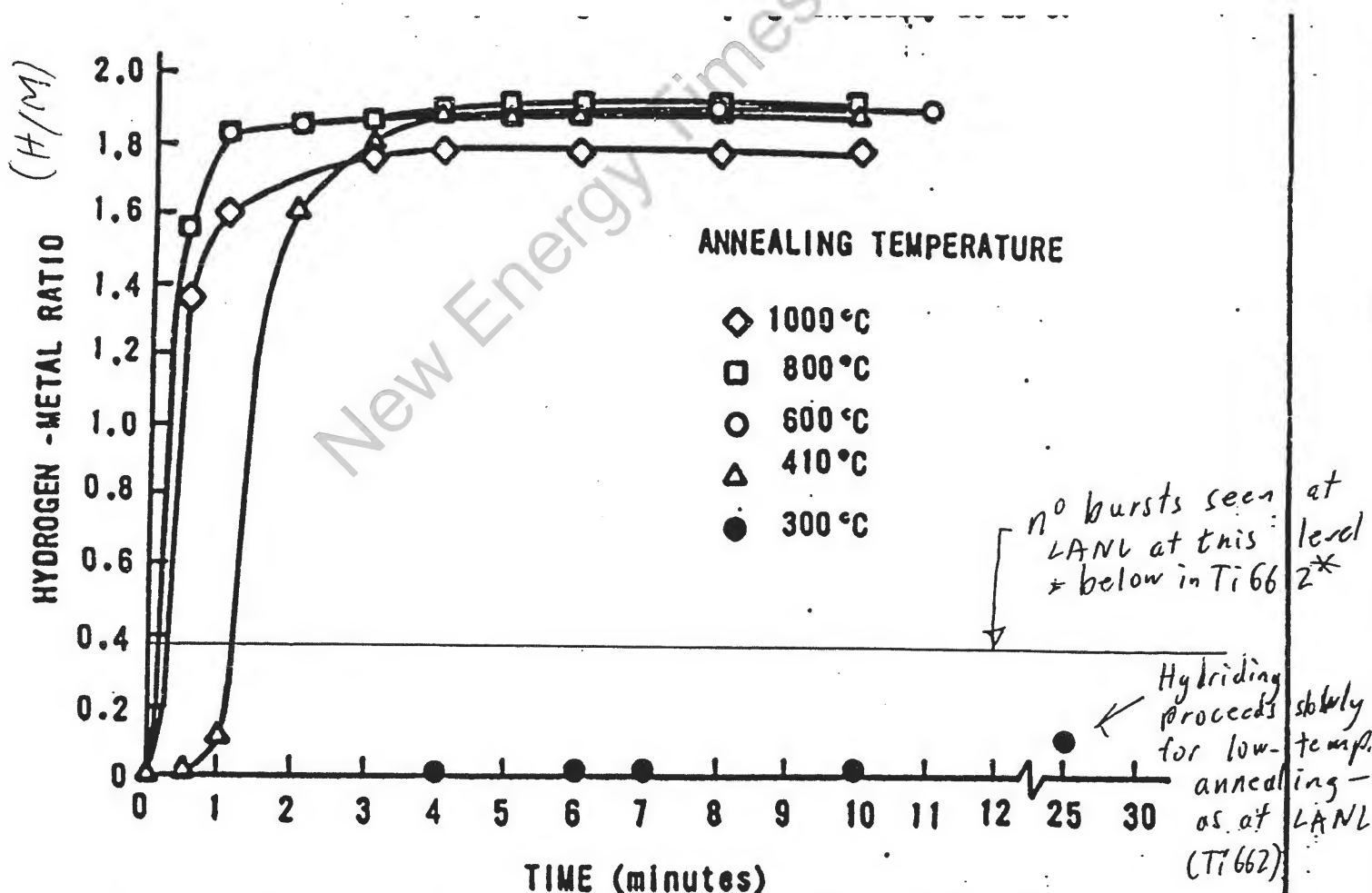


Fig. 3. Formation of titanium hydride from titanium sponge at an initial reaction temperature of 25°C after vacuum annealing samples at various temperatures.

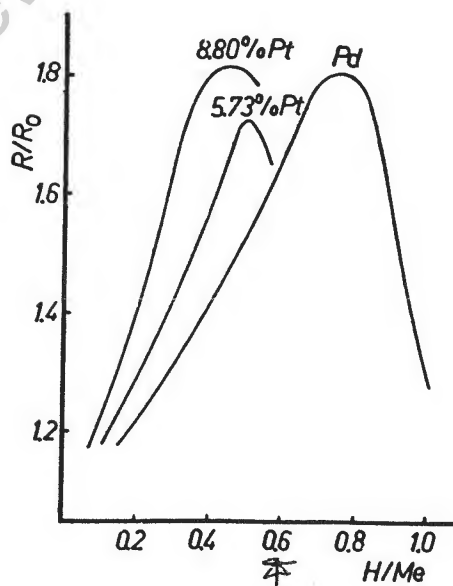
Notice that the H/M ratio increases slowly -- but measurably-- for the case of low-temp. annealing (i.e., below about 400°C), which is the type of procedure used in the LANL experiments. We are explicitly trying to avoid formation of a fully-deuterided material: "we have identified some negative characteristics related to inactive samples such as (1) pre-deuterided Ti or Pd ..." (Update p. 5). In our April 1989 Nature paper we noted: "we have not seen any evidence for fusion in equilibrated, deuterated metals or compounds... non-equilibrium conditions are essential." We have intentionally tried to retain some oxide layer on the titanium so as to take a slow hydriding path as shown by the lower curve in the above data, following low-temp. annealing, rather than annealing the Ti at >400°C which leads to rapid hydriding. Is this clear?

Moreover, we have done MEASUREMENTS on the H/M in both gas-loaded chips and electrodes. Last year, for example, I sent to you detailed information on how sample DH-1 was prepared and used, written up by Mike Paciotti of LANL (attached again here). The deuterium content of the Ti can be evaluated from the recorded pressure drop in the cylinder over the running period, May 20 - June 1, 1989, of (8.5 +/- 2.5) psi. The cylinders hold pressure very well. Since the sample mass was 80g, the AVERAGE deuteron -to- metal ion ratio (H/M) is easily calculated to be 0.2% -- definitely not zero. This information was available to you months ago, although it would take some careful reading to dig it out. Also, I'm sure that I discussed with Moshe over the phone in January deuterium-loading ratios measured by Paciotti in spring 1989; maybe it was Kelvin-- anyway, let's get these results in writing and avoid further annoyance: H/M = 0.002, 0.2, 0.38, (producing neutron bursts), and no bursts from H/M = 0.96, and 1.4 from materials which deuterided rather rapidly (H/M=2 is max. for Ti-D). The graph sent before (shown again above) identified the max. level of deuterium-loading for which neutrons were seen at LANL.

The H/M in the BYU electrodes has been presented at various meetings (e.g., Proceedings of the Oxford Workshop on Muon-Catalyzed Fusion, Sept. 1989). In palladium foil, we measured the increase in resistivity as a measure of H/M:

202

B. BARANOWSKI



from:
Metal Hydrides
ed. G. Bamnolker
NY: Plenum, 1986
p. 202

Fig. 2. Relative electrical resistance of palladium and two palladium - platinum alloys as function of hydrogen concentration at 25°C.

Preparation for Ti 6-6-2.

(Menlove experiment pd-1)

DH-1 : 80g

- 1) Lathe turnings ^{from BYU} with cutting fluid
- 2) Broken up to smaller pieces (small enough for tank entry)
 Condition of the control sample (still have oil)
- 3) Cleaning inside the tank (once we realized that

the cutting oil had been used)

1 May

• Methylene chloride flushes (5-6 flushes)

— no ultrasound —

• methanol flushes (5-6)

• Pure water flushes (5-6)

both can
cause stress-
corrosion

- 4) Pumping & drying with cryopumps at $\approx 50^\circ\text{C}$
- 6) Clean (molecular sieve trap) H_2 flushes to 1000 psi
- 7) Clean H_2 flushes at up to 220°C
 (30 min at 220°C)

8) Cool and fill to 425 psi H_2 at room temp

9) add D_2 to 850 psi (all thru trap)

10) 20 May first cooling LN_2 cycle

21 May another LN_2 cycle

22 May another LN_2 cycle

← Bursts at room temp

- 11) 1 Jun 89 ~~the~~ pressure has dropped by
 $1\% \pm 0.3\%$. Material looks the same
 size to us (no splitting into smaller pieces)

Using a four-point probe and milli-ohmmeter, we found H/M to be about 0.6 by this method. (This was measured about the time that our Nature paper was submitted, and I wish we had included this datum there. Again I emphasize that we have not sought fully-deuterided materials like others including Pons and Fleischmann.)

Measuring H/M in Ti electrodes is difficult because the Ti lattice holds the deuterium tightly. The diffusion rate is low at room temperature, so we expect H/M to be small except near the surface, and we expect the phase boundary to move. (See E. Brauer and R. Gruner, Ber. Bunsenges. Phys. Chem. 187: 341-345, 1983.) We have measured the deuterium in Ti by first drying the cathode in a vacuum at up to 50°C, then by driving off deuterium by heating. The gas is verified to be deuterium (or hydrogen) since it is gettered by Ti (above 400°C). We find H/M = 0.4%, 0.6% (approx., averaged over the entire mass of each cathode) in this way, although this should be considered a lower limit. Again, the amount of deuterium in the cathode is finite. We note that Pd deposited on the surface in our cell might enhance deuterium penetration into the Ti cathode (see e.g. E. Brauer ref. above), and that abrasives are sometimes used to scratch cathode surfaces just before they are used. Also, we use fused or sponge Ti, not Ti foils as used in Paris or Ti "parallepipeds" as used for Yale cathodes. Perhaps the very large surface-to-volume ratio of our Ti cathodes is significant in this context. Moshe states in his 2/18/90 letter referring to the Paris experiment that "the conclusion that deuterium did not penetrate the Ti foils is identical to that of the Yale experiment." Somehow I missed this important point in the Yale Nature paper which reported seeing no neutrons. This is a very significant difference, then, with respect to our experiments where we measured the deuterium content to be greater than zero.

The amount of deuterium in the Ti samples, while greater than zero, is typically far from saturation, intentionally (we seek non-equilibrium conditions, moving phase boundaries, etc.). The data I sent out last year on hydride formation from hydrogen in solution at approx. -30°C (where the neutrons burst rate is also high -- see Update) also involves small concentrations of hydrogen in Ti (e.g. 250 ppm):

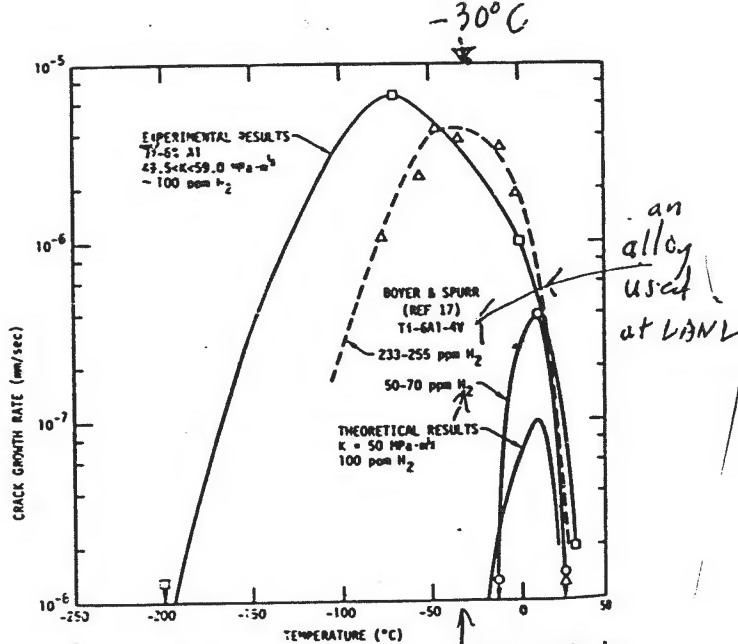


Fig. 5—Crack growth rate plotted vs temperature for Ti-6 pct Al and Ti-6Al-4V (Ref. 17). Experimental and theoretical results have similar functional behavior, but the theoretical peak is smaller and occurs at higher temperature.

W. Purdee and
N. Putton

↑
METALLURGICAL TRANSACTIONS A.

11A: 1390ff.

You have the Ti samples used during the experiments at Yale. I suggest that you measure H/M in these samples and publish the results.

Please be fair to us on these points.

Sincerely,

Steven E. Jones

Steven E. Jones

CC: R. Garwin, P. Parker -- I can't see any point in extending this discussion to a larger audience at this time.

11 MAY 10 11 06 AM '88
Yale 3074-1044
UPPER LIMITS ON EMISSION OF NEUTRONS FROM Ti IN PRESSURIZED D₂ GAS CELLS; A TEST OF THE LANL-BYU EVIDENCE FOR "COLD FUSION"

S.L. Rugari, R.H. France III, B.J. Lund, S.D. Smolen, Z. Zhao, and M. Gai
A.W. Wright Nuclear Structure Laboratory,
Yale University, New Haven, CT 06511

K.G. Lynn
Dept. of Physics and Applied Science, Brookhaven National Laboratory,
Upton, New York 11973

K.W. Zilm
Dept. of Chemistry, Yale University, New Haven, CT 06511

ABSTRACT

We have used a low background detector with high efficiency for detection of bursts to search for emission of neutrons from Ti alloy in pressurized D₂ gas cells (cooled to 77K in liquid nitrogen). Each cell contained between 16 and 67 grams of Ti alloy chips and was prepared by identical methods to those used in a recent Los Alamos - Brigham Young University collaboration of Menlove et al. Three to four cells were used in each experimental run, with a total counting time of 103 hours, leading to an estimate (based on the early reports of Menlove et al.) of at least 3 bursts and as many as 12 bursts expected in our experiment. In a later report the burst rate of Menlove et al. is greatly reduced leading to only one or so burst(s) expected in our experiment. The data was analyzed in two modes. In the first mode (singles mode) all detectors were used to search for neutron bursts with an efficiency of 28% for neutron detection and a background of 100 counts per hour (cph). In the second mode (coincidence mode) the neutron time of flight was measured in a search for random emission with an efficiency of 2% and a background of 2 cph. No statistically significant deviations from the background were observed for correlated neutrons emitted in bursts or for neutrons emitted randomly. All events are shown (with 90% confidence) to be consistent with background. For bursts of neutrons we deduce (with 90% confidence) an upper limit on the bursts' size of 50 neutrons. Our upper limit on the random emission of neutrons, 0.008 n/sec (90% confidence), is a factor of 6 to 25 smaller than the range of rates for random emission above background reported by the Los Alamos - BYU collaboration.

* Supported in part by USDOE contract number DE-AC02-76ER03074.

FROM:

Professor Menlove Gai
Wright Nuclear Structure Laboratory
272 Whitney Avenue
New Haven, CT 06511

For Customer Use

NOT FOR CIRCULATION

DRAFT # Final - 1

Label 11-B (July 1988)

U.S. G.P.O. 1988-195-062-195-36

1) Acceptor may have not broken water
2) gas velocity has not been measured

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not you just

031490..MG

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I INTRODUCTION

Recent evidence for "cold fusion" [1] was reported by Menlove et al. in a Los Alamos - Brigham Young University experiment [2], where bursts of 50-300 neutrons (measured over a 100 μ sec period) were reported from pressurized D₂ cells containing Ti alloy chips ("dry cells"). The cells were cooled to liquid nitrogen temperature and while warming up, were placed in front of the Los Alamos neutron detection system which consists of twelve ³He counters with a total efficiency of $\epsilon=30\%$. It is also claimed that most of the bursts were observed about 30-40 minutes into the warm up cycle at a cell temperature of -30°C, but some of the neutron bursts were also observed at room temperature [2]. In addition, in Ref. 3 it is reported that only 5% of the running time was spent while the cells were warming up, and in Ref. 4 it is reported that only 2% of the running time was spent while the cells were warming up, at temperatures where it is claimed that most of the neutron bursts were observed. Consequently while the experimental study reported here is much shorter (spanning some 10 days), the time spent in our experiment while the cylinders are warming up (22% of the total running time) corresponds to a large fraction of that of Refs. 2-4.

Bursts of neutrons were searched for using six such "dry cells" of essentially identical characteristics to those used in the Los Alamos experiment [2-4]. Three to four cylinders were placed in the detector system during each experiment. We report here the experimental procedure and results of our experiment. The results show no statistically significant deviations from the background for correlated neutrons emitted in bursts or for neutrons emitted randomly.

Our detector system consists of twelve NE213 liquid scintillator neutron detectors [5] that can be operated in two main modes: in singles mode with high total efficiency ($\epsilon=28\pm5\%$) and a moderate background (rate = 100 counts per hour), or in coincidence mode with a moderate total efficiency ($\epsilon=2\pm1\%$) and with low background (rate = 2 cph). Additional tests of other "dry cells" fabricated in

Brookhaven National Laboratory were also conducted in this search with similar results obtained and reported here.

II. EXPERIMENTAL PROCEDURES

II.1 Ti Samples:

The "dry cells" used in this experiment contained between 16-67 grams (each) of Ti662 (Ti with 6% Al, 6% V, and 2% Sn) filings provided by S. Jones and were pressurized to 40-60 atmospheres of D_2 gas. An additional four cylinders contained material from Brookhaven National Laboratory. The cylinders were cooled in liquid nitrogen to 77K and then allowed to warm up to room temperature. Three to four cylinders were used in each experiment as listed in Table I, where we specify the experiments performed. In Table II we specify the cylinders and the details of the Ti material used in each run.

The majority of the data were obtained from cylinders 1, 2, 3, 4, 6, and 8, which are identical to the ones used in Refs. 2-4. The cells were prepared by Dr. S.E. Jones according to procedures developed in the Los Alamos-BYU collaboration. The first three were used over 8 cooling cycles, cylinder 4 was used over 10 cooling cycles, cylinders 6 and 8 over 3 cooling cycles, and the remainder of the cylinders (set K and cylinder 5, containing new samples not used in Ref. 2) were used for 2-4 cooling cycles. All neutron activities reported in the Los Alamos - BYU experiment (see Table III of Ref. 2) started during or before cycle 5, with some 12 bursts occurring during cycles 1-4 and none of the 21 reported bursts [2] beyond cycle 9. We therefore limited the major part of our experimental study to 8-10 cycles per cylinder. In addition, since in Ref. 2 it is claimed that the majority of the bursts occur within an hour after the sample is allowed to warm up, the cycle duration in our experiment was shortened from the 24 hours or so used in Ref. 2, to 4-9 hours, see Table I. While warming up it took two hours for our cylinders to reach 0°C as determined by monitoring the cell's pressure.

Before the Ti662 alloys, used throughout most of the experiments, were loaded into the pressure cells, they were cleaned according to the protocol developed at Los Alamos [2]. This was done to ensure that our samples were as close as possible in composition to those investigated in Reference

2. Since the oxide layer on the alloys is not removed in this treatment [6], it is highly unlikely that a significant amount of deuterium is incorporated into the metal lattice in these samples either through deuteridization or chemisorption. The sample in cylinder 5 (set E) which contained used Ti-Pd alloy electrodes, fused Ti, and Pd was treated to remove the oxide layer, and it did have a significant amount of deuterium incorporated in the solid as evident from an immediate drop in the cylinder pressure and the heat produced, see Table II. This sample was used in two cooling cycles, and when no neutron activity was observed, new samples were prepared. In addition four cylinders prepared in Brookhaven National Laboratory were used, as described in set K.

In a recent collaboration of five Universities at Paris [7] the Ti electrodes in the Jones experiment [1] were studied. It is reported [7] that the deposition of material from the complicated electrolyte used in Ref. 1 prohibited the insertion of deuterium into the lattice. The conclusion of Ref. 7 with respect to the original BYU experiment appears to be very similar to our conclusions with respect to the experiment of the Los Alamos-BYU collaboration.

The background was studied under several different conditions over several different experiments, see Table I (note that background Run 40 is the longest). The background experiments were carried out with no cylinders in the detector system (Runs 30-32), with a lead brick in place of the cylinders (Runs 35, 36), with evacuated cylinders without Ti chips (Runs 40, 41), with a cylinder including only Ti chips under vacuum (Run 51), with cylinders including Ti chips in hydrogen gas cooled to 77K in liquid Nitrogen (Runs 51, 52), and with a cylinder containing deuterium gas with no Ti chips (Runs 51, 52). The cell with just deuterium gas was used to study the possibility of neutrons arising from the photo-disintegration of deuterium.

II.2 Neutron Detector.

The experimental setup is similar to, but more efficient than the one used in the previous Yale-Brookhaven collaboration on "cold fusion". The reader is referred to Ref. 5 for details of the neutron detectors and veto counters as well as the operation of the detector system and the various cuts that can be placed on the pulse shape, pulse height and time of flight (TOF) parameters in the analysis of the data. In Fig. 1 we show the experimental arrangement drawn to scale, with the two central

detectors, U0 and D0, placed 2 cm from the cells and used as scatterers, and the ten ring detectors placed 17 cm from the cells [5].

We list here the various improvements in the system as compared to that used in Ref. 5. Twelve neutron detectors were used in this experiment instead of the six used in Ref. 5. The detectors were arranged in two hemispheres allowing for measurement of up-down asymmetry. In this way we discriminate, in the upper hemisphere, between downward moving (background) neutrons and upward moving neutrons originating in the cells, see below. In addition, the two central detectors are placed closer to the cell's position, see Fig. 1, and have a larger efficiency for events occurring in the cells position, further enabling us to discriminate against background events as we demonstrate below. Three veto counters were used to span a larger solid angle than the two used in Ref. 5. The shielding of the setup was much improved, with the use of some 20 tons of concrete blocks containing iron (of dimension 45x45x60 cm, each).

II.3 Detector Efficiency:

The total efficiencies (intrinsic efficiency times solid angle efficiency) of our neutron detectors were measured using a ^{252}Cf source, placed in the center of the detector system, as well as on the edge of the pentagonal central detectors. We refer the reader to Ref. 5 for the discussion of the energy spectrum of neutrons from the ^{252}Cf source and details of the method for measuring the detector's efficiency using this source. The efficiency of the central detectors was found to be $\epsilon(\text{U0}) = \epsilon(\text{D0}) = 10 \pm 3\%$, and the efficiency of each detector in the outer rings, U1-U5 and D1-D5, was measured to be in the range of 0.5% to 1.1%. We have used the average value of 0.8% for the efficiency of ring detectors in the calculation described below. Thus, when all detectors are used without requiring time of flight coincidences ("singles mode"), a total efficiency of $28 \pm 5\%$ is obtained. The time of flight coincidence efficiency between the central detectors, U0 or D0, and one of their respective ring detectors - Ui or Di ($i=1, \dots, 5$), was measured to be $0.2 \pm 0.1\%$, yielding a total coincidence efficiency of $2 \pm 1\%$.

II.4 Discrimination Against Background:

The efficiency of the central detectors is a factor of 12 larger than that of each of the ring detectors. For a neutron burst of multiplicity M , the probability that one and only one central detector

($\epsilon=0.1$) and at least one out of ten ring detectors (total $\epsilon=0.08$) fires is: $2(1-0.9^M)0.9^M(1-0.92^M)$. The probability that neither central detector fires and one ring detector fires is: $0.8^M(1-0.92^M)$. For a multiplicity $M \geq 50$ ($M \geq 100$) the probability that a ring counter fires without both central detectors firing is at the most 1% (0.005%). For background events not related to the neutrons generated through interaction of cosmic rays with the cylinders, the efficiency of all detectors is essentially the same, allowing a situation where the ring counters fire but not the related central detector. The requirement that the two central detectors fire in a burst (of at least 50 neutrons) is very useful for discriminating against background cosmic neutrons.

The probability for a neutron double hit in one ring detector is given by (a binomial distribution):

$$P_D = \binom{M}{2} \epsilon^2 (1-\epsilon)^{M-2} \quad (1)$$

with $\epsilon=0.8\%$, and for a burst of as many as $M=50$ (100) neutrons originating at the cell's location, the double hit probability for a ring detector is only 5% (15%). The fusion of $D+D \rightarrow {}^3\text{He}+n$ yields 2.45 MeV neutrons which deposit their energy in our detector by scattering off protons in the liquid scintillator. The response of our detector [5] is such that the energy deposited in the detector varies from zero to the neutron energy with equal likelihood. Hence, the probability of observing a pulse height in excess of 2.45 MeV in our detector, as a consequence of a double hit from two neutrons of 2.45 MeV each, is given by:

$$P_D(E) = \int_{x_0}^{\infty} F(x) dx \int_{1-x}^{\infty} F(y) dy \quad (2)$$

where $x_0 = E_{\min}/2.45$, $E_{\min}=E-2.45$, the minimum energy deposited by each neutron, and $F(x)$ is the detector's response function, including its pulse height deficiencies and finite resolution. It should be noted that due to pulse height deficiencies in liquid scintillator the light produced by two recoiling protons, of for example 1 and 2.5 MeV, is summed to yield the light output corresponding to that of a

proton of only 3.0 MeV recoil energy. For example, for a double hit summing up to 3.5 MeV in a ring detector, we find $P_D(E=3.5) = 37\%$ and for a double hit summing to 4.4 MeV we obtain $P_D(E=4.4) = 21\%$. Furthermore, in order that the energy deposited by two neutrons in our detector be fully summed, the two neutrons must hit the detector within the rise time of the detector. The rise time of our neutron detectors is 5 nsec, and a 2.45 MeV neutron takes approximately 5 nsec to travel across the width (10 cm) of the detector. The fast rise time of our detector then leads to an additional correction factor, of at least 0.63, for the double hit probability. The double hit efficiency is then given by: $\epsilon_D \leq 0.63 \cdot P_D \cdot P_D(E)$, and we conclude that the observation of a pulse height larger than 3.5 MeV in a ring detector, can arise from a double hit, with a very low probability, at most 4%, for all possible multiplicities discussed below. For a double hit summed to 4.4 MeV in a ring detector (see below) this probability amounts to less than 2%. We conclude that the knowledge of the energy deposited in the ring detectors is crucial for discriminating against background neutron events.

The efficiency for vetoing cosmic ray related events was measured, as described in Ref. 5, to be of the order of 65% for events occurring in the central detector. In addition, we estimated from our ^{252}Cf source data, that the efficiency for detecting 2.45 MeV neutrons in the veto counters is 0.6%. This source also yields gamma rays which make the measurement of neutron efficiency of the veto counters very hard as the plastic scintillator veto counters do not allow for neutron to gamma discrimination. Using known scattering lengths of neutrons in plastic scintillator material we calculate the self veto efficiency, with the threshold set at neutron energy of 2 MeV (0.7 MeV electron equivalent), to be approximately 0.35%. Thus for a burst of 50 (100) neutrons the self veto efficiency is estimated to be of the order 26% (45%). Therefore, we do not use the veto condition to exclude any data and such events were still analyzed in detail.

For each detector we measured its pulse shape (to facilitate neutron to gamma discrimination) its pulse height (for discrimination against background events, see above) and the time of flight between a start pulse from the central detector and a stop pulse from a ring detector [5]. The time of flight information allows the study of the time ordering of events and the discrimination against downward moving background neutrons. For background neutrons moving downward in the upper hemisphere a

ring counter will fire before U0 which creates a negative time of flight. For neutrons originating in the cell's location we have positive time of flight. This leads to a lower background rate, only 0.4 cph, in the upper hemisphere.

II.5 Gate Length

- An event was accepted only when a signal was registered in one of the two central detectors, U0 or D0. A 20 μ sec wide gate generated from a hit in either one of the two central detectors was used for accepting all data falling within ± 10 μ sec of the trigger (i.e. within 10 μ sec before or after one of the two central detectors fired). For two successive hits separated by 10 μ sec in the two central detectors (or two successive hits in one central detector) the gate duration would then be further extended by an additional 10 μ sec. For example, for a burst including 130(150) neutrons uniformly spread over 100 μ sec (the gate duration of Ref. 2), there is a 94.5%(96.5%) chance of detecting one neutron in either one of the two central detectors out of the 13(15) neutrons during each period of 10 μ sec. The successive detection of neutrons would extend the gate duration. For the above example, there is a 48%(50%) chance of having 13(19) successive hits, each within 10 μ sec of the previous hit, in either one of the two central detectors. On the average these successive hits will be separated by 5 μ sec. Thus the gate duration would be 20 μ sec for the initial hit with an additional 5 μ sec added, on average, for each successive hit. This will then yield a gate duration of 80(110) μ sec during which all pulses will be accepted by our computer. Only at the end of the gate period will our peak searching ADCs start the conversion of analog signals to digital information.

For the time of flight data we used Nuclear Data NIM ADC's (ND581) with conversion dead time of 5 μ sec, for the pulse height data we used Ortec CAMAC ADC's (AD811) with conversion dead time of 80 μ sec, and for the pulse shape data we used Lecroy CAMAC TDC's (LC2228) with conversion dead time of 100 μ sec. The ten Nuclear Data NIM ADC's, reading the time of flight data from the separate TAC's, were interfaced to our CAMAC crate via a fast readout MUX module produced

at Argonne National Laboratory, see appendix A.4.3. The setup is such that two simultaneous hits in the two central detectors arrive at the computer within 50 nsec of each other and in the singles mode the gating is such that no data from any detector is lost over the gate duration.

In the coincidence time of flight mode, the range of the TACs was set to 10 μ sec over 8,192 channels [5], allowing for the study (in the coincidence mode only) of the time structure of bursts spanning 10 μ sec with a precision of 1.25 nsec. The time resolution of the system was measured for γ - γ events to be 2.5 nsec, as discussed in Ref. 5. The detectors were calibrated using standard radioactive sources [5] and the knowledge of the response of the detector to various radiations, as discussed in Ref. 5. The data were taken at the A.W. Wright Nuclear Structure Laboratory at Yale University during the period August 20-29, 1989 and were written onto magnetic tapes event by event for analysis using the various cuts discussed in Ref. 5.

III. EXPERIMENTAL RESULTS

In Fig. 2 we show a typical two dimensional surface plot of pulse height vs pulse shape for ^{252}Cf source data exhibiting good gamma to neutron separation. This separation is obtained even at the low threshold used in this experiment, 50-70 keV electron equivalent (200-280 keV proton recoil energy, hence neutron energy loss).

III.1 First level analysis:

In this analysis a very broad gate is chosen on the pulse shape data (in one dimension) as shown in Fig.2. This broad gate makes certain that no neutrons are excluded, while at the same time it includes a considerable number of background gammas (and background neutrons), see below. For the background runs, more than 90% of the pulse shape signals which survive the gate are gamma rays. In Figs. 3a and 3b we show a histogram of the number of detectors fired (event fold) using the liberal gate on the neutron pulse shape, shown in Fig. 2. In this analysis multiple successive hits of neutrons in one detector within our gate are counted as a fold $K=1$ event. Only four events were observed to have high-folds in the whole array (including hits in both central and ring detectors), $K=5$ or 6. Two were

observed in separate background runs (Runs 40 and 51) and two were observed in separate data runs (Runs 45 and 61). All 4 high-fold events were vetoed by the cosmic ray veto counter as shown in Fig. 3.

III.2 Second level analysis:

In this analysis we placed cuts on the two dimensional spectra shown in Fig. 2. In addition we require that both U0 and D0 fire (see above) and applied a cut on pulse height to exclude events for which more than 3.5 MeV energy is deposited in a ring counter (see above). Note that the probability for a chance coincidence between a cosmic neutron (rate = 100 cph) and a neutron from a burst is of the order 10^{-6} , for a gate width of 20 μ sec, justifying exclusion of an event in which one of the ring counters registers a pulse height larger than 3.5 MeV. Using this analysis, no events of total fold $K > 3$ are observed (i.e. no more than one ring detector fired) in any of the runs, see Figs. 3c and 3d.

In Fig. 4 we show the time distribution of the four high-fold events, as obtained with the first level analysis using the less restrictive one dimensional gate on the pulse shape. The time calibration is 0.9 min per channel, and $t=0$ is a few minutes into the warmup cycle. Note that in the data shown in Figs. 3 and 4 we also observe the same rate, within uncertainties, for folds $K=4$ and lower, as in the background runs. Indeed, no statistically significant deviations from the background were observed for low-fold events in any of the runs.

The list of observed event parameters for the four high-fold events of the first level analysis is given in Table III. Three out of four of these events have pulse shapes which are on the border of our (one dimensional) pulse shape gate and are accepted here only due to our wide gate. In only one event (during Run 45) did we observe only pulse shapes expected for neutrons, and only in this event did both central detectors U0 and D0 fire. As shown above, the probability for a burst of at least 50 neutrons from fusion during which one of the central detectors D0 or U0 does not fire is less than 1%. As can be seen in Fig. 4, the event of Run 45 occurred one hour and twenty-one minutes into warmup cycle 4 for cylinders 1-3 (cycle 3 for cylinder 4) at a temperature of approximately -10°C as deduced from the cell's pressure. In this event detector U5 registers at least 4.4 MeV, but the probability of observing 4.4 MeV in a ring detector was shown to be less than 2% and we conclude that this event most likely arises from

cosmic neutrons. As can be seen in Table III, in all four of these high-fold events the energy deposited in at least one ring detector is of the order of 4.4 MeV, in excess of the 2.45 MeV expected from a fusion event. We thus conclude (with at least 90% confidence) that all high fold events are consistent with background events. It is worth noting that the detectors (D5 and U5) that register a large energy deposit from cosmic neutrons are located nearer to the less shielded front side of the setup which has only paraffin shielding without concrete, see Fig. 1.

Inspecting the time scale and energy deposited in detectors U5, U1 and U0 in the event of Run 45 listed in Table III, we may suggest that the event is consistent with a high energy ($E > 7.3$ MeV) cosmic-neutron (arriving from the less shielded front side, see Fig. 1) that undergoes triple scattering. It starts with one large angle scattering from detector U5 to its immediate neighbor, detector U1, depositing a large energy (4.4 MeV) in detector U5. The neutron then travels its mean free path in liquid scintillator (a few cm) in about one nsec and undergoes a small angle scattering in detector U1 towards the central detector U0. From U1 it travels for 17 nsec to the central detector U0. This time period is listed in Table III as negative, since in this case the ring counter was hit before the central detector. Note that a neutron with a kinetic energy of approximately 2 MeV travels, on average, for some 15 nsec between a ring detector and a central detector. It appears that the parameters of the high fold event of Run 45 listed in Table III, can be explained by this hypothesis of one high energy neutron executing triple scattering between detectors U5, U1 and U0.

III.3 Upper Limits:

We have calculated the fold probability in our array using the formalism first developed by B.R. Mottelson as reviewed in Refs. 8-10. The fold probability in our array of twelve detectors is determined by the 10 ring detectors; therefore, we only consider the fold probability in the ring counters. For an array of ten (ring) detectors the fold probability, $P_K(M)$, of observing K ring detectors fired (K fold event), for a neutron burst of Multiplicity M is given by:

$$P_K(M) = (-)^{10+K} \binom{10}{K} \sum_{i=1}^{10} (-)^{i+1} \binom{K}{i} [1 - (1-i\epsilon)^M] \quad (3)$$

Where $\binom{N}{I}$ is the usual binomial coefficient (equal to zero for $I > N$), and $\epsilon=0.8\%$ is the efficiency of a single ring detector. Note that equ. (14) of Ref. 8 is missing the binomial coefficient $\binom{10}{K}$, which however is correctly inserted in equ. (15) of Ref. 10. Detailed Monte Carlo simulations [11] confirm the validity of equ. (3).

The total efficiency of the 10 ring detectors is given by $1 - \epsilon'$, where $\epsilon' = (1-10\epsilon)^M$ is the inefficiency of the array. Note that the array's inefficiency is given by a fold $K=0$ event (i.e. $\epsilon' = P_{K=0}$). For example, for a multiplicity of 25 (35) the total efficiency of our ring counters is 88% (95%). We note that for a given event the sum of all fold probabilities for folds $K=1, \dots, 10$ must equal $1 - \epsilon'$. This would be required since we either miss the detection of the event or observe a 1, 2, 3, etc. fold event, with an efficiency of $1 - \epsilon'$, (i.e. $\sum_{K=0}^{10} P_K(M) = 1$).

In Fig. 5 we show the fold probability of our array calculated using Mottelson's exact solution [8], equ. (3). For an event of multiplicity $M=100$, the array's total efficiency is 99.98%, and the most likely event is of fold $K=5$ or 6 (i.e. 7 or 8 neutrons detected in the entire array). No such events were observed in any of our experiments.

Since we have at least 90% probability for observing a fold $K=4$ or larger event (i.e. 6 or more neutrons detected in the entire array), see Fig. 5, the first level analysis (using a very liberal gate) yields to an upper limit on the burst size of 100 with 90% confidence. As shown in the event parameters listed in Table III, this analysis is clearly allowing a large number of background events to be accepted. Having established a first level analysis upper limit of 100, we use our previous estimates for double hits etc. with $M < 100$ (see section II.4), and we apply the cuts of the second level analysis. As discussed in section II.4 the probability of removing a true fusion neutron event through the use of our cuts is less than 5%. We estimate that for multiplicity $M > 50$ we have at least a 90% probability of observing a $K=2$ or larger fold event (i.e. 4 or more neutrons detected in the entire array). Since no

valid events are observed of more than one fold in the ring detectors, in our second level analysis, see Fig. 3c, we deduce (with 90% confidence) an upper limit on the size of neutron bursts of 50.

Based on the early data of Menlove et al. [2] we expect, see appendix A.3, as many as 4.8-12 neutron bursts of 75-300 neutrons over the duration of our experiment. As discussed in the appendix A.3 we note that the burst rate reported in the early work of Menlove et al. [2] was much reduced in subsequent reports [3,4], and using the later data we only estimate on the average 1 or so bursts to occur in our data. For an expected N bursts, the probability that a burst will in fact occur in our experiment is $1-e^{-N}$, see below. For an expected occurrence of two bursts in our experiment it is 86%. We then conclude that our data rules out with 90% confidence the evidence for "cold fusion" as reported in the early work of Menlove et al. [2]. In order to test the later results [3,4] one may need to run for as long as two months to rule out this later, lower rate.

In Fig. 6 we show the rate above background of neutrons emitted randomly. For these data the time of flight coincidence method was used [5] which yielded the very low background of 2 cph (0.4 cph in the upper hemisphere and 1.6 in the lower hemisphere). Using the backgrounds measured in Runs 40, 41, 51, 52, and 60, we deduce the neutron rates above background shown in Fig. 6, which exhibit no significant deviation from the background. The ensemble average of all our data yields the rate: -0.2 ± 0.4 cph above background. We then deduce (with 90% confidence) the random emission of neutrons not to exceed 0.6 cph, corresponding to a neutron source of 30 neutrons/hour (0.008 n/sec). This upper limit is a factor of 6 to 25 smaller than the observed range of rates above background reported in Ref. 2.

IV CONCLUSIONS

We have searched for the emission of neutron bursts from Ti samples (as similar as possible to the ones used by Menlove et al.) using an array of neutron detectors with high efficiency for the detection of several neutrons from a large burst. No statistically significant deviations from the background were observed, and we obtain upper limits on neutron emission, in both burst and random emission modes, which are significantly lower than the evidence for "cold fusion" reported by Menlove et al.

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We thank R.L. Garwin for offering his intriguing solution for fold probability as outlined in our appendix A.1. We also thank Dr. Garwin for several illuminating discussions and bringing to our attention Ref. 3 of the Los Alamos - BYU collaboration. We thank J.E. Hack of Yale University and J. Reilly of Brookhaven National Laboratory, for help in preparing our samples, and we thank J.R. Beene for useful conversations.

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APPENDICES

A.1 Fold Probability:

In deriving equ. (3) standard calculations of fold probabilities are used [8-10]. In the following we outline a somewhat different and more elegant solution provided to us by Dr. R.L. Garwin. We however, emphasize that the two solutions agree within a few percents.

For an array of N detectors of efficiency ϵ each and a neutron burst of M neutrons, the expected average number of hits in a counter is $M\epsilon$. The striking of a detector by neutrons is a Bernoulli (binary) process with M trials with a success chance of ϵ each. The consequence of this process is a certain number of hits (I) in a detector with a probability given by the binomial distribution [13]:

$$P(M, \epsilon; I) = \binom{M}{I} \epsilon^I (1-\epsilon)^{M-I} \quad (4)$$

In the limit of small value of ϵ , this probability behaves like the Poisson distribution [13]:

$$P(\mu, K) = e^{-\mu} \mu^K / K! \quad (5)$$

with the mean hit number in a detector of $\mu = \epsilon M$. For example, for $M=50$ we find a double hit probability ($I=2$) of 5%, as deduced previously from equ. (1). Therefore the probability that no hits occur in a detector is: $e^{-M\epsilon}$, and the probability that a detector is struck by neutron(s) is: $1 - e^{-M\epsilon}$. If a detector has only one vote independent of the number of hits in it, only these two values are of significance. The probability that none of the N detectors in our array are struck (fold $K=0$) is then: $P_0 = (e^{-M\epsilon})^N$, and the probability that precisely one (ring) detector fires (fold $K=1$) is given by: $N(1 - e^{-M\epsilon})/e^{-M\epsilon} * P_0$. The probability for a fold K event is then:

$$P_K(M) = \frac{N}{(K)}(1-e^{-M\epsilon})/e^{-M\epsilon} * P_0 \quad (6)$$

Equation (6) is equivalent to our solution equ. (3) and gives identical results to those shown in Fig. 5. Equation (6) is however more easily applied for a general array (with detectors of different efficiencies) and yields a more transparent solution.

A.2 An approximation for the fold probability:

As discussed in Ref. 8, equ. (3) could be approximated by:

$$P_K(M) = M * 10\epsilon * (M-1) * 9\epsilon * \dots * (M-K+1) * (10-K+1)\epsilon / k! \quad (7)$$

Care should be used in using the approximation, equation (7), as it is only valid for probabilities smaller than 50% (for large M, it is in fact larger than 1). We note that use of this approximation led us to conclude in a preliminary report [12] an upper limit on the burst size of 27. The upper limit deduced in this paper using the exact solution of equ. (3) is 50.

A.3 Estimate of Expected Number of Bursts:

In order to estimate the number of bursts expected in our experiment a detailed description of the observed burst statistics, running time, down time etc. of the Los Alamos - BYU experiment is needed. These data have not been made available in any of the published material of the Los Alamos - BYU collaboration. Thus, we have to rely on partial report of these data [2-4] and private communications from Dr. S.E. Jones. In addition, as outlined below, there is much inconsistency in reported results of the various partial reports of the LANL-BYU collaboration.

The Los Alamos - BYU collaboration reports two different burst rates. Early in their experiment Dr. H. O. Menlove reported at the International Workshop on Cold Fusion, held at Santa Fe on 20 May 1989, at least 12 bursts of 75 - 255 neutrons occurring between 28 April and 20 May with at least 6 of these bursts including at least 130 neutrons. These data were also reported in the first version of Ref. 2 that covers the same time period. All 12 of these bursts were observed during warm up time; therefore, it is claimed [2-4] that the warm up period is the crucial one for observing bursts.

Hence, we only consider the warm up time in the following estimates of the burst rate. During this time period at least 12 bursts of 75-255 neutrons were observed over 23 days of data acquisition.

Experiments using four detection systems over four and a half months are described in Refs. 3 and 4. But in Refs 3 and 4 we find only 20 bursts of 50 or more neutrons observed while the cylinders are warming up (10 such bursts observed at room temperature). Out of these twenty bursts, ten were in excess of 130 neutrons. While the early reported burst rate is one burst per two days [2], the later reported rate [3,4] corresponds to one burst per seven days.

If we make the upper limit assumption that the down time for the experiment was zero and that data were collected over all days (including weekends) with the use of all four systems without interruption for background measurement, calibration, service of detectors etc., we are lead to the estimate of 2,200 hours in the early period [2] and 4,104 hours used over the four and a half months [3,4]. We emphasize that in Ref. 2 we find that at least during part of the time, at the initial stages of the experiment, some systems were used for measurements of background. Hence, this time estimate of 2,200 hours for the early period provides only an upper limit on the useful running time of the Los Alamos - BYU experiment. While it is claimed [14] that running time of the later period corresponds to 13,000 hours running, we can only find published accounts [3,4] of 4,104 hours. Despite repeated requests for data supporting the larger number of hours (13,000 hours) which we made to the LANL-BYU collaboration, we were unable to confirm the larger number (13,000 hrs) and are thus adopting the published later running period of 4,104 hours.

In Ref. 3 it is reported that during only 5% of the running time were the cylinders at temperatures below 0°C, warming up from 77K. In contrast, the warmup time is listed as only 2% of the total time in ref. 4. Due to this confusion we will use both figures in our estimates. We then estimate the useful running time at warm up temperatures to be at least $(2,200 \times 0.02 =)$ 44 hours and at most $(2,200 \times 0.05 =)$ 110 hours for the early period [2] and at least $(4,104 \times 0.02 =)$ 82 hours and at most $(4,104 \times 0.05 =)$ 205 hours for the later period [3,4]. Note that this running time estimate includes the assumption that all 4 systems were running concurrently and therefore does not correspond to "real time".

In our experiment from a total of 88 hours of running with Ti662, approximately 20 hours (of "real time") occurred during the warm up period (neglecting the running time with samples made at Brookhaven). On the average, four samples were used over 10 cycles each with 2 hours at temperatures below 0°C.

On the average, the Ti samples used in the Los Alamos - BYU collaboration contained 83.7 grams of Ti chips per cylinder (see Table II of Ref. 2) or 82.3 grams per cylinder (see Table II of Ref. 3 and addition to it by Dr. S.E. Jones). In our experiment we used on the average 4 cylinders containing a total mass of 184.4 grams of Ti chips.

While in the Los Alamos - BYU experiment approximately 30% of the neutrons in a burst are counted, in our data we would detect approximately 10% of the neutrons in a burst, see Fig. 5. Nevertheless, the efficiency for measuring neutrons from a burst in both experiments is in excess of 95%.

This leads to four estimates of the expected burst rate in our data. Based on the early reports [2] we estimate $(20/44 \times 184.4/83.7 \times 12 =)$ 12 bursts or $(20/110 \times 184.4/83.7 \times 12 =)$ 4.8 bursts to occur in our data and based on the later reports [3,4] we estimate $(20/82 \times 184.4/83.0 \times 20 =)$ 11 bursts or $(20/205 \times 184.4/83.0 \times 20 =)$ 4.3 bursts on average in our experiment during the warm up period. If we were to use the figure of 13,000 hours [14] for the later running time, then the burst rates over the later period would be reduced by a factor of 3.

In the above estimate we made the reasonable assumption that the burst is spread over a time scale shorter than our gate duration (of minimum 20 μ sec). If we assume that the burst is uniformly spread over the gate duration of 100 μ sec (as listed in Ref. 2) or 128 μ sec (as listed in Refs 3-4) a correction factor should be added. For example, as discussed in section II.5 for a burst of 130(150) neutrons uniformly spread over 100 μ sec our setup has a 48%(51%) chance of having a gate 80(110) μ sec long, see above. Note that half of the neutron bursts observed at Los Alamos included at least 130 neutrons. We then renormalize our overall observation efficiency by 0.44(0.65) for detecting such extended bursts including at least 130 neutrons (i.e. overall renormalization of 0.22(0.325) for the

expected burst rate). In the Los Alamos experiment, due to the neutron die away time in their detector of 50 μsec [3,4] their detector would catch only 86% of a burst extended to 100 μsec . In this case this would then lead to the estimate of at least 1.2 and as many as 3 bursts in our data, based on the early data of Menlove et al. [2] and, on the average, at least 1.1 bursts based on the later data of Menlove et al. [3,4]. Not knowing the duration of these bursts, we conclude that we should have observed at least 1 and possibly as many as 12 neutron bursts in our experiment.

A.4 Experimental Systematics:

In the following we shall discuss possible improvements to our setup that may allow an increase of the sensitivity of our experiment, but do not invalidate our results (as suggested in Ref. 11) at the quoted level of sensitivity.

1. Our detector system is designed to catch a small fraction of the burst, for example for a burst of 100 neutrons we show that we have at least 90% efficiency for detecting a 6, 7, 8, 9, or 10 fold event. Detection of a larger fraction of neutrons could be carried out via an increase in the number of detectors, but not by an increase in the individual detector efficiencies. An increase in the efficiency of each detector would increase the double hit probability (kept smaller than 4% in this experiment) and not allow for discriminating between a hit from a high energy cosmic neutron and a simultaneous multiple hit of fusion neutrons summing to more than 2.45 MeV in the detector. We emphasize that such simultaneous hits can not be resolved even via the use of a multiple hit electronics owing to the finite time resolution (of a few tens of nsec) of such devices.
2. Due to the low threshold used in this experiment, the gain of our detectors must be high, causing some of our detectors to saturate at energies greater than 4.4 MeV neutron energy. As we demonstrated above, the probability for such an occurrence in our data from a fusion event is smaller than 2%. Clearly this saturation does not affect our experiment at the quoted level of sensitivity.
3. Cross talk was observed between different time of flight spectra. We emphasize that time of flight information is not used in any way to identify a burst and the cross talk does not

interfere with our ability to demonstrate the occurrence of bursts. The cross talk disappears (greater than 99.5%) upon minimal pulse shape gating (i.e. requiring a pulse shape signal above threshold) in the appropriate ADC channel as shown in fig. 7. The cross talk is due to data read more than once in the buffer of the electronic module, MUX-PRW103, interfacing our Nuclear Data ND581 NIM ADC's to our CAMAC crate. We note that all ten NIM ADC's are multiplexed and interfaced to the CAMAC crate via this single memory buffer. In addition, since the data in each ADC is read more than once, the clean time of flight spectrum for a given detector is reduced by a factor close to 2 with respect to the ungated data.

4. The time of flight between the two central detectors was not measured in this experiment. This fact does not affect our ability to search for bursts, since the signals from the two central detectors were timed with respect to each other so that hits in both detectors are included in the gate.

5. For relatively small bursts ($M < 100$) of duration exceeding 20 μsec , an increase of the gate duration used in this experiment would be highly desirable. As described above, for large bursts ($M > 100$) our produced gate duration of 20 μsec is extended to values as large as 80 μsec or more and the short gate duration used in this experiment is not so crucial.

REFERENCES

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Table I: Summary of Runs

Run Number	Type of Run	Cylinder Set	Length of Run
3	PuBe Neutron Source	None	0:03
4	Cs 137 Source	None	0:01
5	Co 60 Source	None	0:01
11	Cf Source	None	0:40
12	Cf Source	None	<7:48
14	Cf Source	None	0:10
20	Cf Source	None	0:07
21	Cf Source	None	<0:10
22	Cf Source	None	0:02
23	Signal Timing Calibration	None	0:19
30	Background	None	0:02
31	Background	None	0:20
32	Background	None	8:16

At this point we adjusted on-line gates, pulse shape discriminators, and the voltages of detectors U0 and D0

33	Co 60 Source	None	
34	Cf Source	None	2:16
35	Background	1/2 Lead Brick*	1:20
36	Background	1/2 Lead Brick*	0:23
**			
40	Background	Set L	8:57
41	Background	Set L #13	3:33
42	Data Run	Set A #1,2,3	6:15
43	Data Run	Set A	12:30
44	Data Run	Set B	9:22
45	Data Run	Set C	9:46
46	Data Run	Set D	7:23
47	Data Run	Set D	7:53
48	Data Run	Set E	9:47
49	Data Run	Set E	6:26
50	Cf Source	None	2:57
51	Background	Set F	7:30
52	Background	Set G	4:06
53	Data Run	Set H	4:03
54	Data Run	Set I	4:02
55	Data Run	Set K #9,10,12	4:01
56	Data Run	Set K #9,10,11	4:30
57	Data Run	Set K #9,10,12	4:00
58	Data Run	Set K #9,10,11	4:25
59	Cf Source	None	1:00
60	Background	Set M	4:01
61	Data Run	Set J	8:44

Notes:

*

A 2"x4"x3.5" lead brick was placed between detectors U0 and D0.

**

Run 40 followed Run 36 directly. There were no runs labeled as Run 37, Run 38, or Run 39.

Table II: Cylinder Sets Used in Experiments

Cylinder Set	Cyl #	Type of Gas	Type of Metal
Set A			
	#1	D2 @ 780 PSI	60g of thick Ti662* chips [2]
	#2	D2 @ 990 PSI	66g of thin Ti662 chips [2]
	#3	D2 @ 680 PSI	67g of Ti662 chips ***
	#4 **	D2 @ 250 PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips ***
Set B			
	#1	D2 @ 720 PSI	60g of thick Ti662 chips [2]
	#2	D2 @ 980 PSI	66g of thin Ti662 chips [2]
	#3	D2 @ 680 PSI	67g of Ti662 chips ***
	#4 **	D2 @ unknown PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips***
Set C			
	#1	D2 @ 680 PSI	60g of thick Ti662 chips [2]
	#2	D2 @ 980 PSI	66g of thin Ti662 chips [2]
	#3	D2 @ 680 PSI	67g of Ti662 chips***
	#4 **	D2 @ unknown PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips***
Set D			
	#1	D2 @ 660 PSI	60g of thick Ti662 chips [2]
	#2	D2 @ 980 PSI	66g of thin Ti662 chips [2]
	#3	D2 @ 680 PSI	67g of Ti662 chips***
	#4 **	D2 @ unknown PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips***
Set E			
	#2	D2 @ 980 PSI	66g of thin Ti662 chips [2]
	#3	D2 @ 680 PSI	67g of Ti662 chips***
	#4 **	D2 @ unknown PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips***
	#5	****	
Set F			
	#6	H2 @ 790 PSI	65g of Ti662 chips [2]
	#7	D2 @ 790 PSI	No Chips
	#8	Vacuum	65g of Ti662 chips [2]
Set G			
	#6	H2 @ 730 PSI	65g of Ti662 chips [2]
	#7	D2 @ 780 PSI	No Chips
	#8	Vacuum	65g of Ti662 chips [2]

Set H

#4 **	D2 @ unknown PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips***
#6	D2 @ 860 PSI	65g of Ti662 chips [2]
#8	D2 @ 860 PSI	65g of Ti662 chips [2]

Set I

#4 **	D2 @ unknown PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips***
#6	D2 @ 850 PSI	65g of Ti662 chips [2]
#8	D2 @ 850 PSI	65g of Ti662 chips [2]

Set J

#4 **	D2 @ unknown PSI	12g of thin Ti662 chips [2] and 4g of Ti662 chips***
#6	D2 @ 860 PSI	65g of Ti662 chips [2]
#8	D2 @ 860 PSI	65g of Ti662 chips [2]

Set K

#9	D2 @ 400 PSI	50g of FeTi
#10	D2 @ 414 PSI	45.9g of V(D2)
#11	D2 @ 415 PSI	20.3014g of Y metal
#12	D2 @ 200 PSI	24.98g of La(D3)

Set L

#13	H2 @ 780 PSI	Empty
#14	Vacuum	Empty
#15	Vacuum	Empty
#16	Vacuum	Empty

Set M

#17	Vacuum	50g of FeTi
-----	--------	-------------

Notes:

- * Ti662 is an alloy of Titanium containing 6% Al, 6% V, and 2% Sn
 ** This cylinder was smaller than the others and lacked a pressure gauge.
 *** Material from Ormet Corporation
 **** This cylinder contained :
 36g of Ti(80%) - Pd(20%) from powders sintered together
 21.0g of Ti(90%) - Pd(10%) from powders sintered together
 25.7g of Ti - Pd - V from powders sintered together
 7.8g of fused Ti
 5.0g of Pd
 These materials were used as electrodes in previous experiments on "cold fusion" in electrochemical cells. Upon exposure to an initial 100 PSIG of D2 gas, these electrodes immediately underwent a hydriding reaction as evidenced by a sharp rise in the temperature of the pressure cell. The reaction had gone to completion before counting and no additional D2 gas was added.

Table III: Summary of High Fold Events

Detector	Energy Deposited (MeV)	Time of Flight (nsec)	Pulse Shape
Five Fold Event in background Run 40:			
U0	2.8	0.0	n
U2	0.9	3.7	n
U4	0.9	3.7	n or γ
U5	4.4	-2.4	n or γ
D2	1.5	N/A	n
Five Fold Event in background Run 51:			
U3	0.4	N/A	n or γ
U4	2	N/A	n
U5	4.4	N/A	n or γ
D0	0.9	0.0	n or γ
D1	1.5	3.7	n or γ
Five Fold Event in data Run 45:			
U0	1.8	0.0	n
U1	1.1	-17	n
U3	0.6	3800	n
U5	4.4	-18	n
D0	3.3	N/A	n
Six Fold Event in data Run 61:			
U1	0.6	N/A	n
U4	0.4	N/A	n
D0	5.4	0.0	n or γ
D2	1.6	740	n or γ
D4	1.1	1.2	n or γ
D5	4.2	1000	n

FIGURE CAPTIONS

Fig. 1: A schematic diagram of the experimental arrangement drawn to scale.

Fig. 2: Typical two dimensional pulse height vs pulse shape plot for neutron detector U1. In a first level (more liberal analysis) the very broad one dimensional gate was used, as shown in the projection along the pulse shape axis. Events listed in Table III as neutron or gamma events, have pulse shapes that fall within 10 channels of the gate's limit (around channel 140). Note the good neutron to gamma separation even at this low threshold.

Fig. 3: Total number of detectors fired (fold): (a) and (c) for data runs and (b) and (d) for background runs, with or without a veto condition from the cosmic-ray detector. In (a) and (b) we show the fold pattern obtained from the first level analysis (using the liberal gate shown in Fig.2), and in (c) and (d) we show the results of the second level analysis, as discussed in the text. All four high fold events, shown in (a) and (b), are vetoed.

Fig. 4: Total event fold (in the entire array), as a function of time into the warm up cycle, using the liberal gates of the first level analysis. The time calibration is 0.9 min per channel, and $t=0$ is a few minutes after the cylinders were removed from the liquid nitrogen bath.

Fig. 5: The fold probability $P_K(M)$, for the ring detectors, calculated using equ. (1).

Fig. 6: Neutron count rates above background, for random emission. The ensemble average corresponds to the rate of -0.2 ± 0.4 cph above background. Runs 1-36, 50, and 59 are setup and calibration runs.

Fig. 7: Time of flight spectra; in (b) and (d) we show the crosstalk discussed in section A.4.3, and in (a) and (c) we show the clean spectra generated with minimal pulse shape gates.

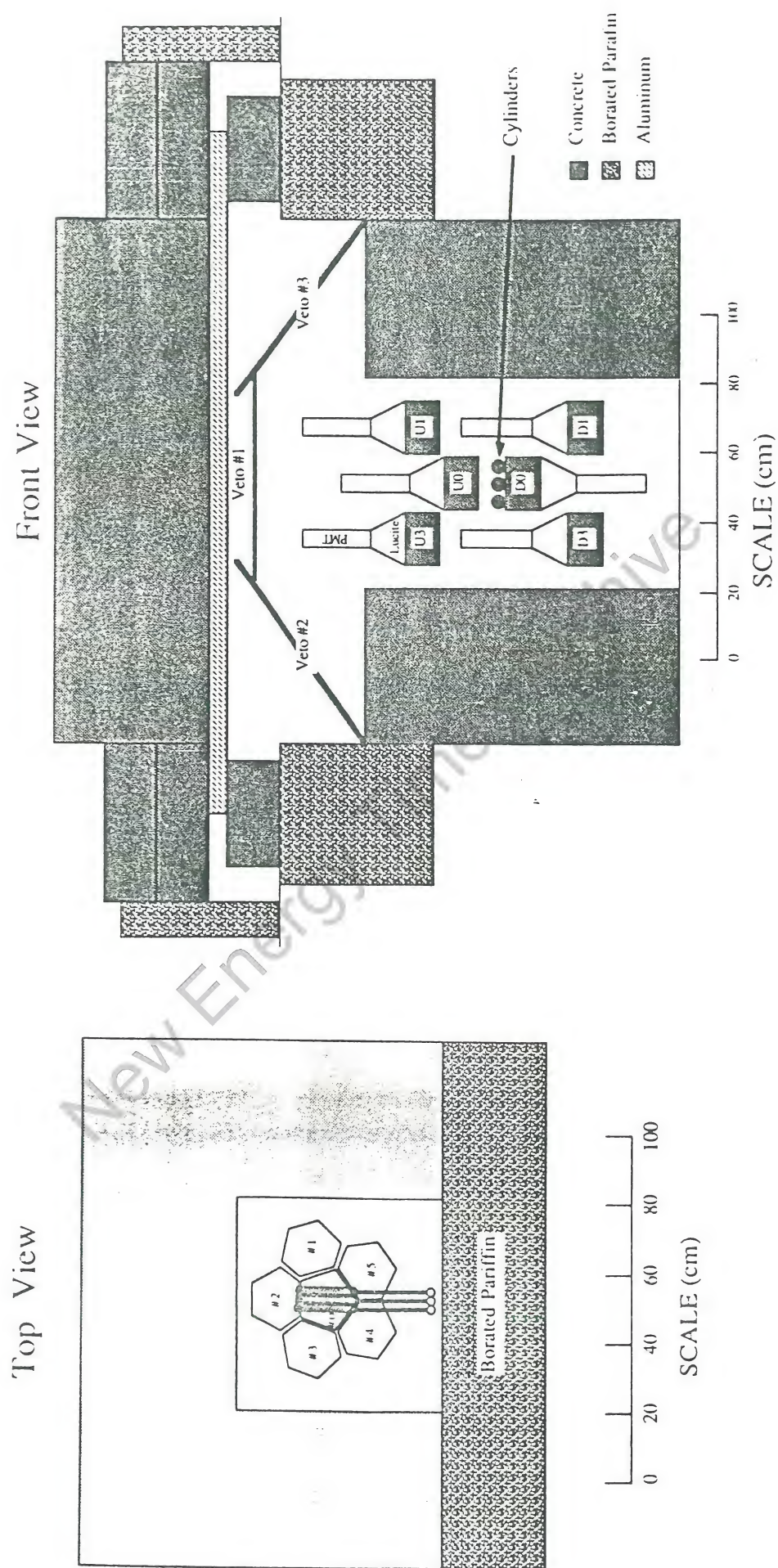


Fig. 1

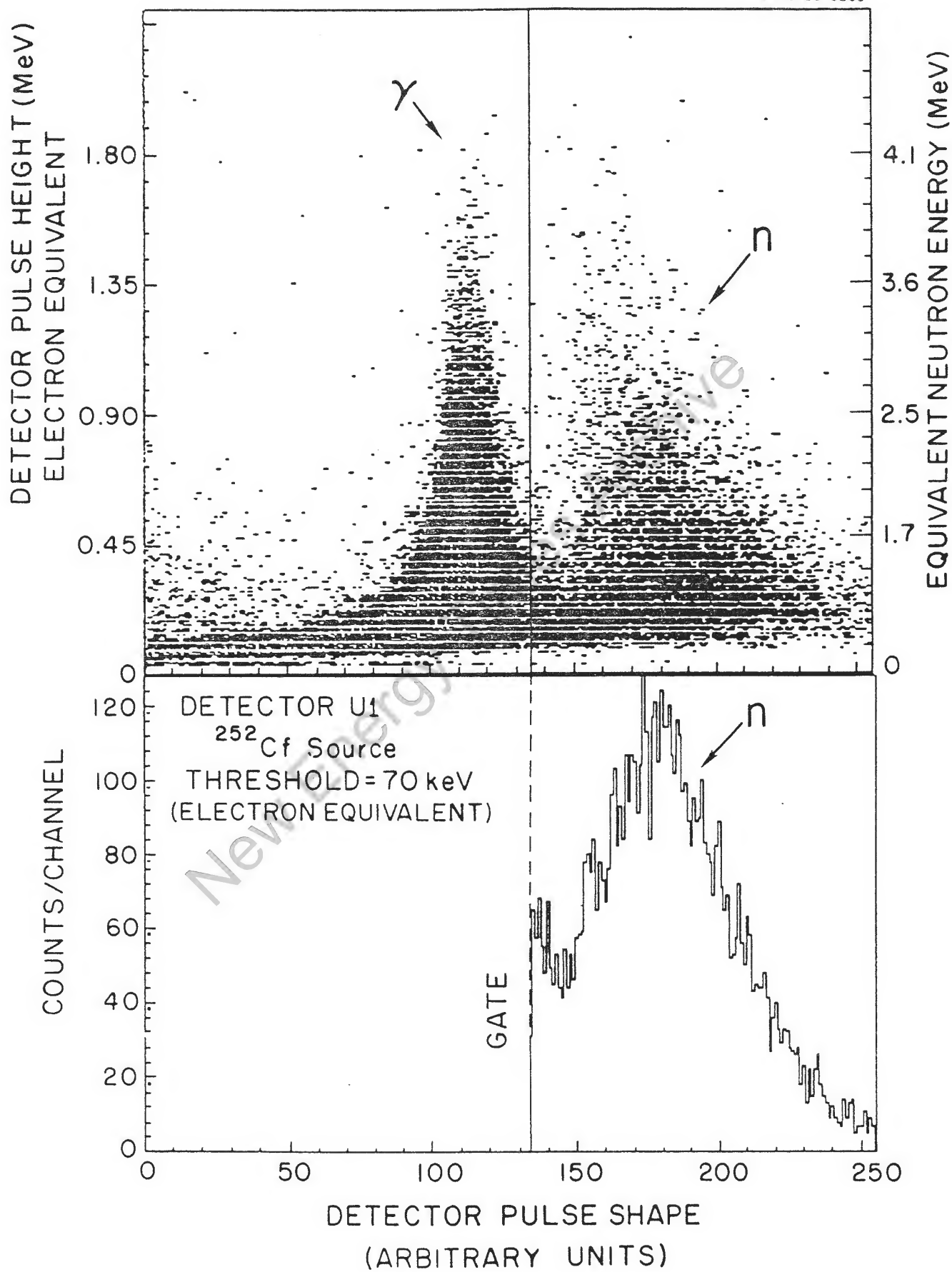


Fig. 2

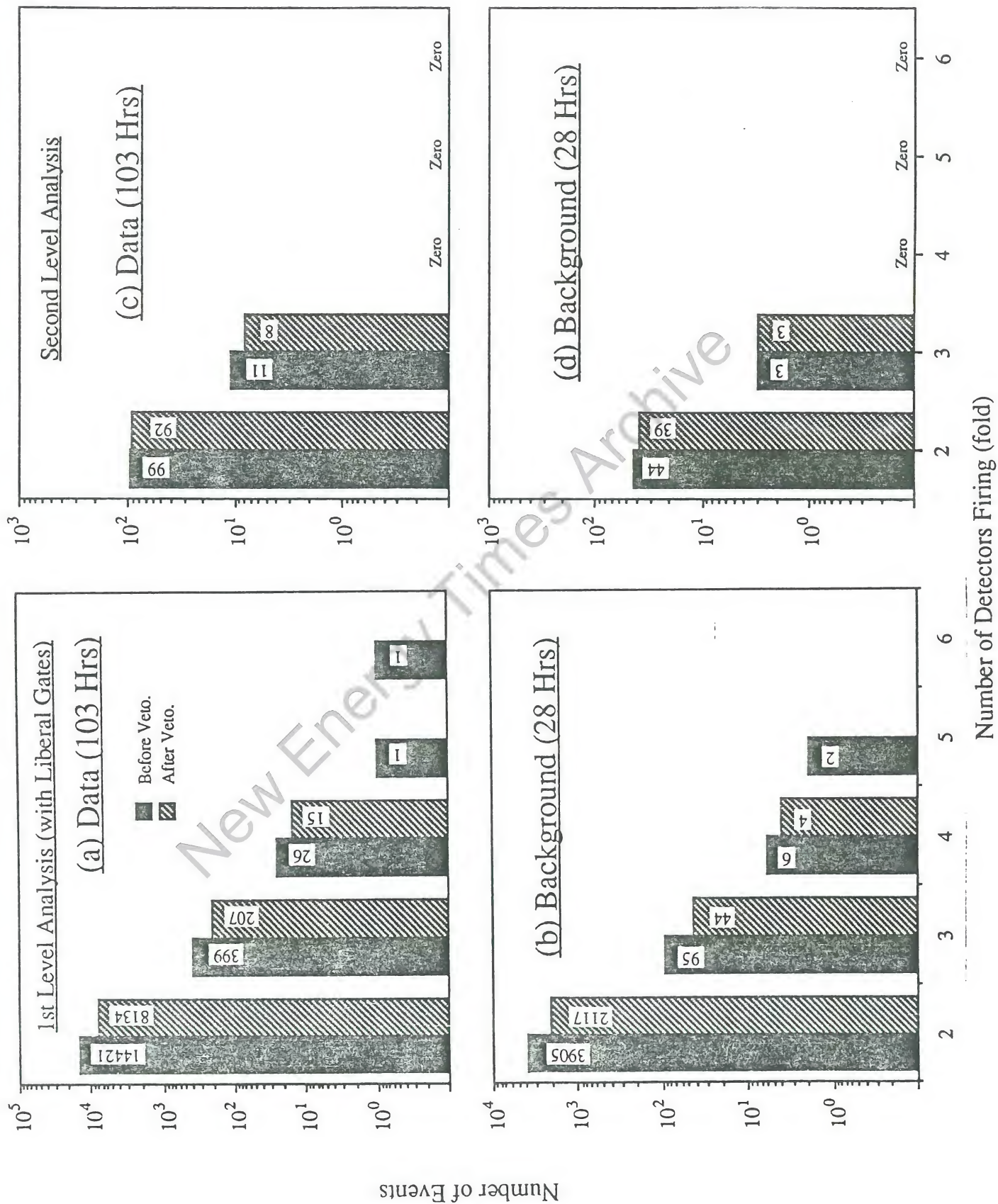


Fig. 3

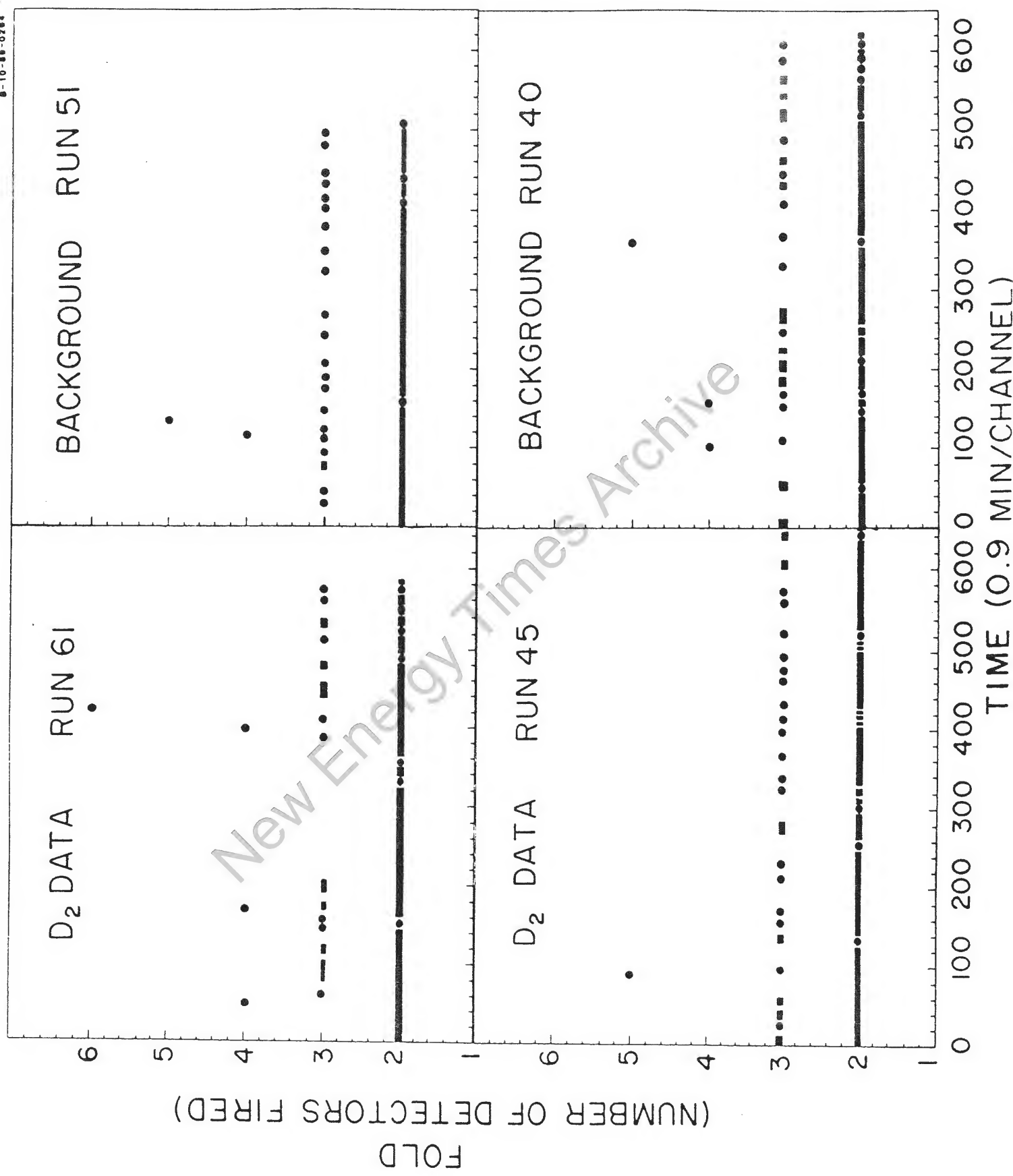


Fig. 4

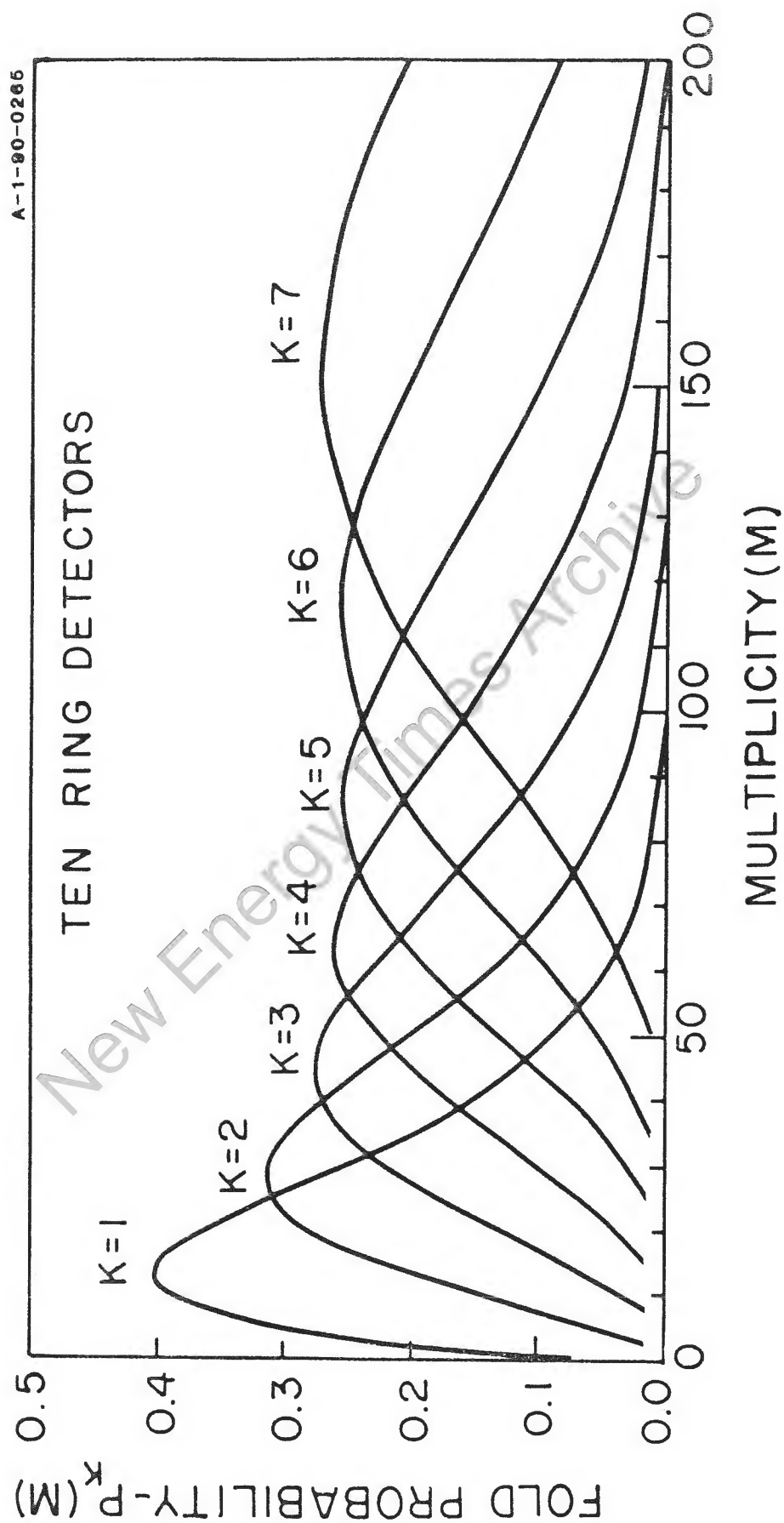
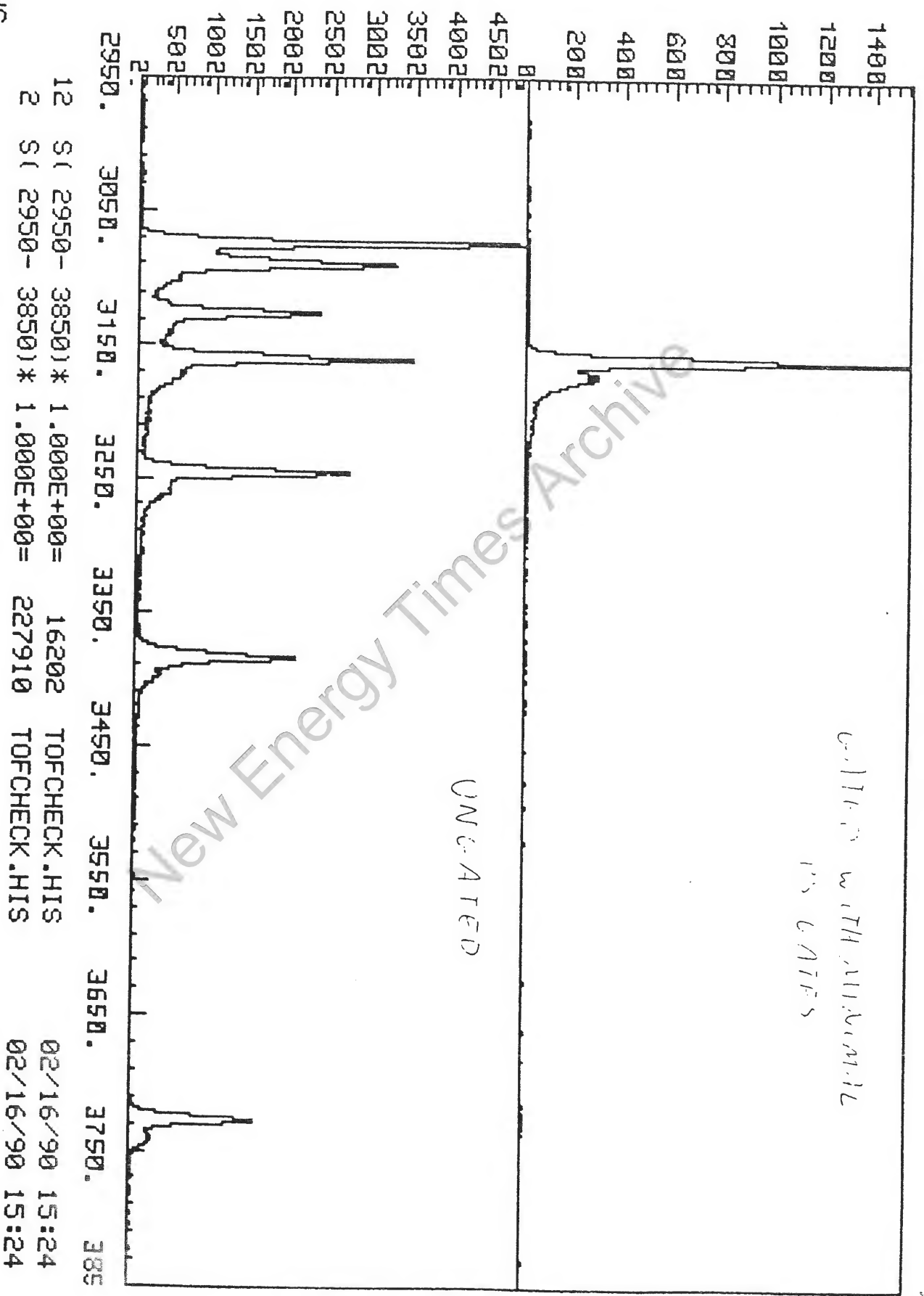


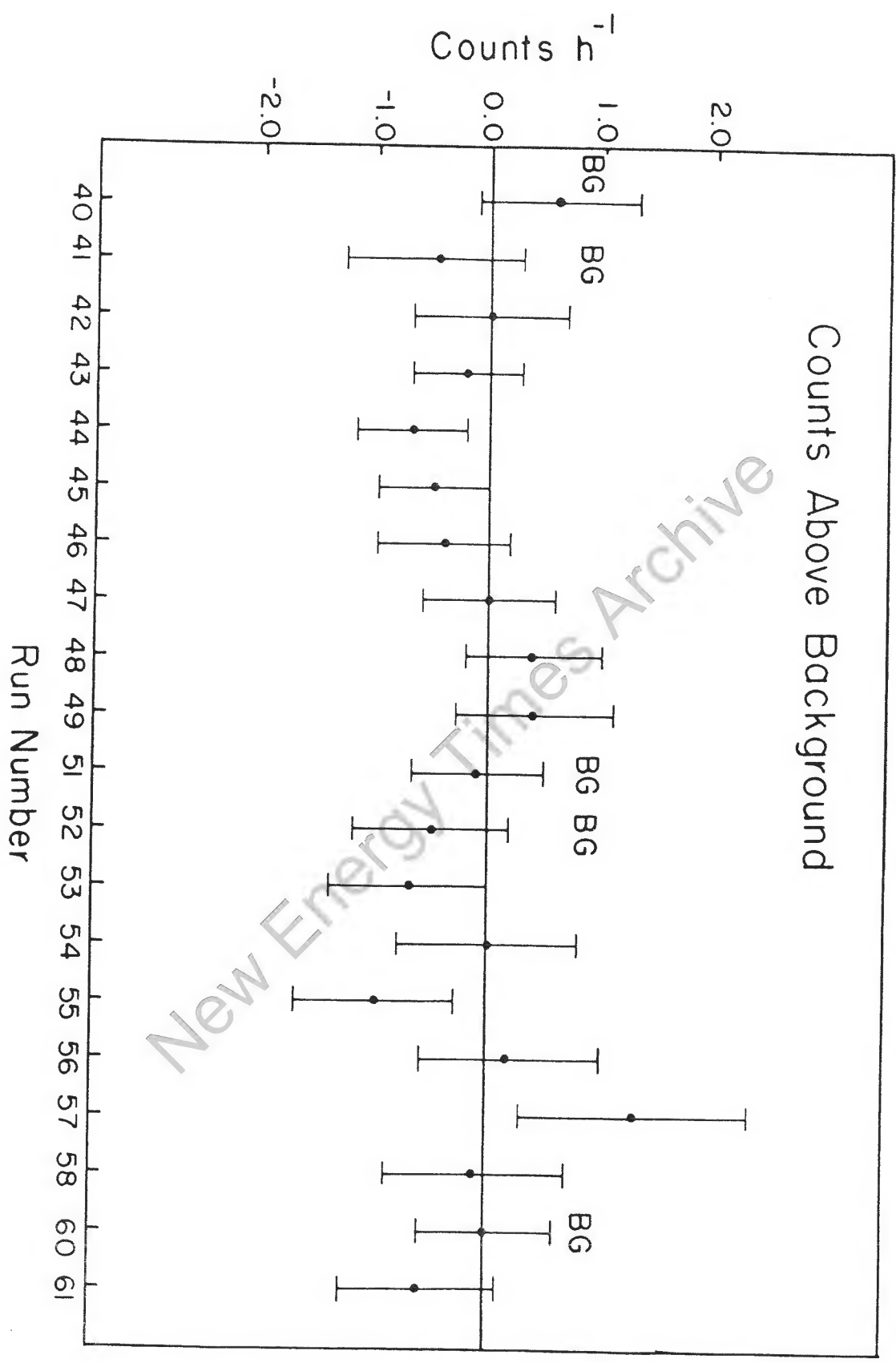
Fig. 5

Fig 7

(to be completed, soon)

XHC





Date: Fri, 16 Feb 90 18:17 CST
From: <DROEGE@FNAL>
Subject: The A. Bruggerman et al paper.
To: RLG2@YKTVMV
Original_To: JNET%"RLG2@YKTVMV"
Original_cc: DROEGE

Garwin
Droege
Morrison

From your 11 Feb. message:

"I later received a preprint from a laboratory abroad which had discovered "excess heat" traceable to an oscillating power supply"

I also note your use of this paper to influence national policy.

I do not believe the content of the paper supports it's title. To save me the several days effort required for a point by point rebuttal of the paper, perhaps you might re-read it. I am not able to find any place where they say that they produced the same power levels in the laboratory that they say on the anomalous event.

They make a model, then say that by putting rather large capacitors in the right places, also resistors, that they can make it oscillate.

They do not say that they then measured their apparatus and found the similar values of R and C. In fact, if I understand their model correctly, then by the time they put in a big enough resistor to allow the oscillation, there would be so much power in it that it would be easy to tell what was happening - the excess resistance would get very hot.

Of course I do not believe the experiment as one that say "anomalous heat". (there are now two say's where their should be saw in this memo.) From time to time I get such an event. Like the time I turned off the scope after looking at the cell voltage. The resulting transient set a bit in one of my DAC's. The resulting display got my brother a phone call in the middle of the night. But since I record the DAC outputs as one of the data stream items it was all explained the next day when I looked at the data.

There is just no content in the paper that would allow figuring out what really happened. I could have saved them the effort of the simulation by telling them that there is such a thing as a phase shift oscillator. I would more bet on some screwy power supply connection where one DC supply tries to drive the other since on page 8 they comment that the "potentiostats" were warmer than usual. But it could have been an oscillation. We will probably never know.

If this paper had been by someone who claimed that their was "fusion" I bet you would have torn it to shreds. (there) Since it supported your view, it was probably easy to toss it on the "good paper" pile.

This note is rambling, but there is a delicate path to be followed. Both pro and con papers need to be evaluated critically and fairly. Should we publish data that is not rock solid? I am presently trying to figure out how to present my data to the NCFI. I have measurement of excess heat for which I am quite confident. I have other measurements which will be expensive, if not impossible for me to confirm without more facilities. Yet these could save/cost others a lot of work. How do I talk about them?

I like your idea of actually looking at the cell voltage. I challenge you to actually do it. For someone who is shaping the national policy, it should not be asking too much of you to actually build two cells and to look at them. I bet you \$100 against your 2 cents (I just discovered to my amazement that my IBM keyboard has no cents key!) that you will agree it was worth doing. All you have to do to collect is to say that you looked and it was not worth the effort.

Any time you wish, you can use my (cheap) scope in my basement. I just need a day or two warning since I am on a tight schedule of alternate H2O and D2O runs.

Again a plea for good humor. This is fun. You seemed a little defensive in sending me your view graph. I am quite confident in your measurement ability. I followed with interest your work on the gravity waves.

Everyone says it can't be fusion because there are no neutrons. I believe that neutrons are scarce in this experiment. I don't care if its Fusion, Fission, or Pfooy. It's Fun, Fun, Fun, until we can explain it. It is sure not like CDF where everything we do is dull and hard work. This experiment has so many mysteries that there is always something exciting to try. Right now I have a 20 sigma radiation correlation. I have a test running to try to prove it is junk. If this one doesn't do it I am out of ideas! Last week we thought to put film in the calorimeter. It turned black with funny images where we had put absorber. Pretty exciting until we did the matching experiment and found we could turn our film black by running at the same temperature 1000 miles away. Still got images of the metal absorbers!

Just trying to make sense,

Tom

=====

Date: Mon, 12 Feb 90 14:40 CST
From: <DROEGE@FNAL>
Subject: Your 22 Feb. visit
To: rlg2@watson
Original_To: JNET%"rlg2@watson"
Original_cc: DROEGE

I will look for you on the 22. I am about 2 miles away from the main building in the "old directors complex" but I will find you if given a chance. Extension is 3286. I am a late worker so I will surely be here at the end of your visit day. Since I am working on a paper for the NCFI meeting I should be able to give you a draft by then. By basement is about as close to where you will be as my office. But there is nothing really to be gained by a visit that conversation will not reveal. But you would be more than welcome to visit.

Could I ask you to forward back to me a copy of my last message. I am trying to keep a complete file on this "adventure" and you were not accessible from my usual computer so copy_self was not set.

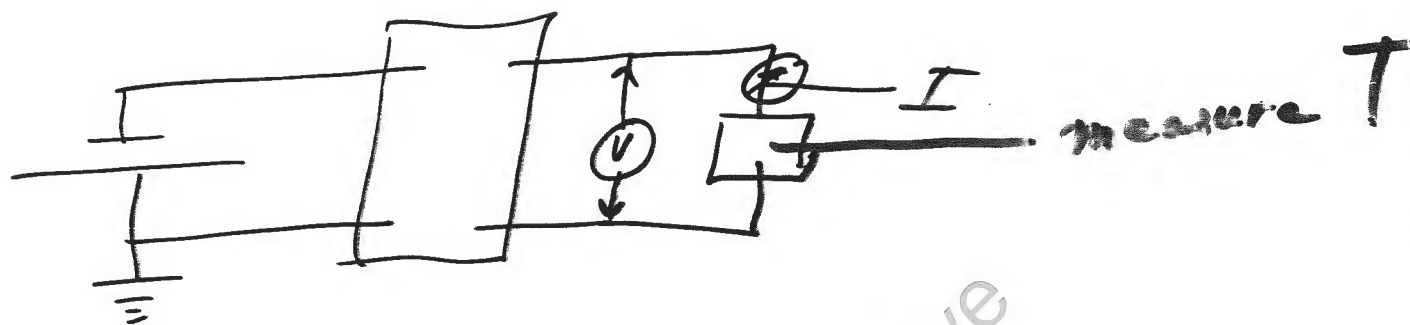
Thank you

Tom

New Energy Times Archive

05/12/89

Not even so easy to calculate [1]
"power in"!



I have put in $I = 0.1 \text{ A}$

$V = 200 \text{ V}$

and dissipated $200 \text{ W} \gg V \times I$

$$\bar{P} = \langle V(t) \cdot I(t) \rangle$$

If V is constant $\bar{P} = V \langle I(t) \rangle$

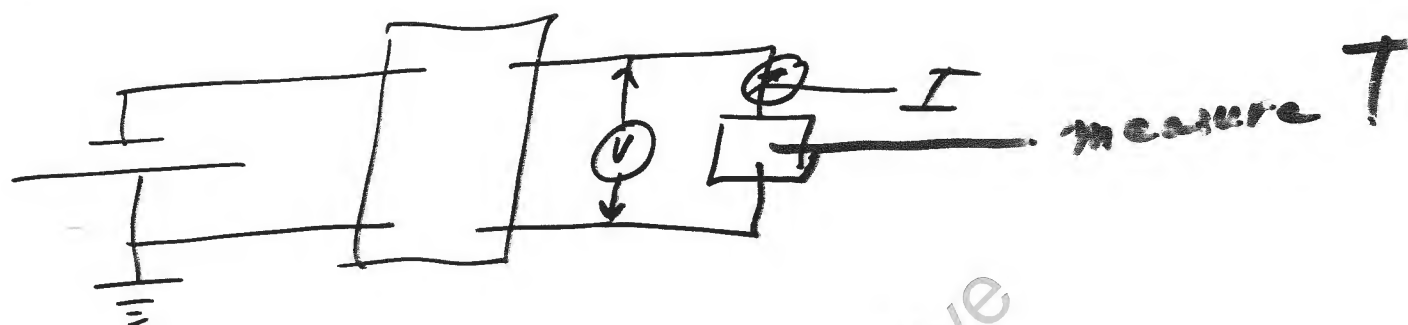
If I is constant $\bar{P} = \langle V(t) \rangle I$

But if not, $\bar{P} \neq \langle V(t) \rangle \times \langle I(t) \rangle$



05/12/89

Not even so easy to calculate ^[1]
"power in"!



I have put in $I = 0.1 \text{ A}$

$V = 200 \text{ V}$

and dissipated $200 \text{ W} \gg V \times I$

$$\bar{P} = \langle V(t) \cdot I(t) \rangle$$

If V is constant $\bar{P} = V \langle I(t) \rangle$

If I is constant $\bar{P} = \langle V(t) \rangle I$

But if not, $\bar{P} \neq \langle V(t) \rangle \times \langle I(t) \rangle$



=====

Date: Mon, 12 Feb 90 13:42 CST
From: <DROEGE@FNAL>
Subject: "Cold Fusion Experiments"
To: rlg2@watson
Original_To: JNET%"rlg2@watson"

I am honored that you have taken time to read my stuff. I assume that Douglas sent you everything, but am willing to make clarifications.

I hope you were not offended by my using your name in referring to V*I. It turns out there is a lot to see and you have put me on to it. I run with a constant current supply. The cell voltage then looks like a saw tooth. There are very pronounced differences between H2O and D2O cells.

As soon as I get a chance, I will write some code that takes cell voltage vs time at high speed, I don't have a scope camera at home. Right now, I am too excited looking at an unexpected many sigma effect in my radiation counter. Probably just systematic junk, but it is hard to stay calm as the effect gets larger as I add up more old runs.

Back to using your name. This field is too serious for my taste, and there has been too much name calling. I like to think we are all trying to solve a puzzle, and that most of us are doing honest work. At the time I first mentioned your name in the Morrison correspondence, I was a little miffed at having to investigate one more thing where I thought I knew what I was doing. As it turns out, my measurement technique was OK, but it was still worth looking, and I thank you.

I am having a wonderful time. I wish I could somehow communicate this to some young people. The school system does an awful job of explaining what science is about. As does the media. My own daughter was turned off from a science career by a Physics teacher who is honored by his school as a great teacher but who taught such a rigid science course that my daughter rebelled.

Best wishes,

Tom Droege

Date: 11 February 1990, 13:08:53 EST
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: DROEGE at FNAL
cc: Douglas R.O. Morrison (41-22) 767-3532 DROMCD at vxcern.decnet.cern.ch
Subject: Cold fusion experiments.
Reply-To: RLG2 at WATSON

Douglas Morrison has recently sent my your correspondence, which I have read with great interest and respect for the careful and perceptive experiment which you have been conducting.

I notice my own name appearing at times, and tomorrow I will send you copies of the foils I used in briefing the National Science Board, end-May, and at a meeting 04/12/89 and at other times regarding the problem of $I \times V$. Of course I mentioned very prominently that if V were regulated at the cell, any variation of I would still allow $\langle V * I \rangle = V * \langle I \rangle$, and if I were regulated then $\langle V * I \rangle = \langle V \rangle * I$. I later received a preprint from a laboratory abroad which had discovered "excess heat" traceable to an oscillating power supply.

Certainly this is no insurmountable obstacle, but one must keep one's eyes open as you have clearly been doing.

I will be at Fermilab for a brief visit Thursday 02/22/90 and would like to meet you, if only for a few minutes. I doubt that I would have time to see your apparatus, but would like to see pictures, sketches, etc. How do I contact you at Fermilab? I will visit with a few other IBM people in connection with future computing and will be seeing John Peoples and others.

Best regards.

Dick Garwin

Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 5675; Thu,
08 Feb 90 10:46:30 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Thu, 08 Feb 90 10:23:53 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA07369; Thu, 8 Feb 90 10:21:35 +0100
Message-Id: <9002080921.AA07369@dxmint.cern.ch>
Date: Thu, 8 Feb 90 10:23 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

From:
43028::DROEGE 7-FEB-1990 21:11:41.36
To:
22719::MORRISON
CC:
DROEGE
Subj:
Repairs to last message

First few lines of the first paragraph should read:

Just for fun I added it up. I have done 49 runs with resistors
in oil or air, or Pt-Pt cells in H2O totaling 735 hours. I have
done 23 runs with Pt-Pd in H2O cells totaling 690 hours. I have
61 runs with Pt-Pd in D2O cells totaling 3000 hrs. I am now
running cells ...

Just got a note from bj who dislikes E-mail as much as I do.
Actually I like the concept. There was a period - say from a
about 1950 when people used the telephone instead of writing
letters. Biographers will have a terrible time. What would
we know of Mozart if not for his letters? Now there will be
E-mail to leave a nice trail of ideas. (and send yor to jail if
you do a scam!)

Best regards,

Tom

=====

Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 5589; Thu,
08 Feb 90 10:44:43 GVA
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Thu, 08 Feb 90 10:21:44 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA07330; Thu, 8 Feb 90 10:19:34 +0100
Message-Id: <9002080919.AA07330@dxmint.cern.ch>
Date: Thu, 8 Feb 90 10:21 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

From:
43028::DROEGE 7-FEB-1990 20:58:17.53
To:
22719::MORRISON
CC:
DROEGE
Subj:
Cold Fusion

I wonder if blank lines will work as well?

Dear Douglas,

Just for fun wsrs
in oil or air, or Pt-Pt cells in H2O totali7ours. I have
do
ne 23 runs with Pt-Pd in H2O cells totaling 690 hours. I have
61 runs with Pt-Pd in D20 cells totaling 3000 hrs. I am now
running cells #8, #9, #10 and #11. All but the last eleven runs
are junk. But that is the way of this business. I have worked
in high energy physics long enough to know that the only good
data comes on the sunday before the machine is shut down
permanently.

I would be honored to have Garwin look at my work. An E-mail
address would help me make contact.

So far, in all the data, I have only one matched triplet of
resistors in oil, Pt-Pd in H2O and Pt-Pd in D20. Because of the
CO2 problem most of the other runs are beyond recovery. My
present plan is to alternate between H2O and D20 runs between now
and the NCFI conference to which I have submitted a paper. I
should be able to make a pair of runs a week. After three pairs,
I plan to switch the electrolyte between the two cells. This is
risky as I will not be able to get all the H2O out of the cell
which gets the D20 electrolyte. I will need to do this as the
cells are not quite identical. We shall see whether it is only
a calorimeter paper or whether the data is good enough to report!

I am reading bar resistance, gas evolved, 16 electrical parameters, and a radiation count. So far, there are very obvious differences between the H2O cell and the D2O cell when excess power is seen. I do not rule out a chemical process, but it will need to require D2O. Something is going on and I can see it! I very much need to find someone to share my data and think about it. I am good at staying up all night and keeping balky apparatus functioning. I am not so good grinding stuff through computers as you can tell from my reluctance to learn how to send files.

One reason that the D2O runs are usually made longer is that one "expects" the effect to go away if it is chemical. So these runs are usually made as long as the cell voltage can be held. The really troublesome thing is that the cell voltage always goes up during the run, though others seem to report both directions. This means that runs cannot go "forever" which is necessary to prove something other than a chemical reaction.

In summary here are the differences:

1) At least five times the "anomalous heat" with the D2O cell than the "drift rate" for an oil-resistor or H2O cell. I am rapidly accumulating data now that I have gotten rid of the CO2 problem.

2) The gas servo is at least ten times as "noisy" with the D2O cell. With H2O the gas servo takes a step (1/70 cc) every few minutes. With D2O it is constantly pumping in and out of the sample. Peak numbers are .5 cc - 35 steps per minute, with a guess at an rms of 8-10 steps.

3) The "Garwin" number moves further from one with D2O than with H2O. This means I under measure the excess heat the way I compute it. I think this is another way of saying the cell is noisier.

4) The cell voltage is noisier with D2O than with H2O. Again a factor of 5-10 in rms value. Observations of the saw tooth wave form look different. My view is that the bubbles are frequently blasted off by something.

5) A very small correlation between the size of the gas change and the count in my 1" NaI(Tl) detector. This is probably one more statistical fluctuation, but it is large enough that I will have to do a very long run alternating between cell current on and off at something like 10 hour intervals to shoot it down.

6) Accumulation of typically 25000 joules during a D2O run where the sequence is 12 hours warm up in the calorimeter before data taking starts, then 12 hours at zero cell current, 48 hours at current, and 12 hours at zero current. This is compared to an H2O run which is forced to zero. I will be able to state this better when I have a few more runs.

I find that my paper has been accepted at NCFI so I hope to see you there. Again a plea for help. With good analysis I may have something to say other than "here is a neat way to build a calorimeter."

Some answers to your questions. Mostly I have tried to give you the flavor of what I am doing. Data for the NCFI paper will be more carefully done. Yes, I have runs where there is excess heat in a Pt-Pt cell before closing the water trap and much less afterwards. Almost everything must be redone, and while the data is clear to me it will not be convincing until I do it again. This particular data is flawed in that the open cell data was taken zero current, cell current, zero current while the closed run was taken only at cell current. It was also run in H₂O. So I do not have a Pt-Pt run that is directly comparable with an Pt-Pd in D₂O.

I cannot correlate the power peaks with any other effect. I do not even know when the power peaks occur. What I actually measure is that the cell voltage goes down for a while. This results in less input power to the cell. This seems to be because the bubbles are blasted off more frequently. I know that the long term heat balance is maintained, because that is what the servo system does. As far as I can tell the "anomalous heat" could come at any time, even with cell voltage peaks, the low voltage periods simply there to keep the balance.

The present experiment is operating in a constant temperature enclosure. It is just not a dewar. I have a nice stainless steel dewar now but my computations indicate it will not be much better than my copper and styrafoam design. Just neater.

Thanks for the word on Alvarez. I have always told my wife that the way to get me to take a vacation in some exotic spot is for me to get on an experiment like Alvarez's pyramid x-ray scheme.

I hope you realize that I send these long notes as much for my own benefit as yours. It helps me to organize my thoughts when I try to present them to an informed reader. I have now told you everything I have to say so I won't bother you until I have something new to report.

Thank you for your attention.

Tom

Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 5703; Thu,
08 Feb 90 10:46:39 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Thu, 08 Feb 90 10:25:10 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA07403; Thu, 8 Feb 90 10:22:38 +0100
Message-Id: <9002080922.AA07403@dxmint.cern.ch>
Date: Thu, 8 Feb 90 10:24 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion. Hope you do not mind me giving him your address?
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear Tom,

8 February 1990.

Thanks for your long message. Particularly enjoyed the opening and closing paragraphs. Like you I am not a computer expert, but do find Email so important that it is worth an effort.

Was very impressed by the large number of runs you had done, but then read on and learnt that most were junk! Suppose you had been under pressure from friends and bosses to publish/give a press conference? However in our branch we have learnt that early experiments often need modification. A friend told me that after he writes a paper, he puts it in a drawer and does not look at it for a month, then he reads it again with a fresh mind (this is Guiseppe Cocconi who you may know).

Comparing H2O and D2O is not straightforward as there must be small differences - think I wrote about them in one of my notes. Such as different resistivities so that one cannot simply say that one should make the current or voltage the same and then expect any difference attributable to "an effect". It is worth reading the DOE Panel report, especially Appendix 2C.

It is curious that you are observing effects in D2O and less in H2O such as "noise" of the gas servo and of the cell voltage. How could one check that this is due to the D2O and not to another effect - when you interchange the cells this will help. Are there other ways to prove the effect is caused by something else? - you could also change the electrodes, the voltmeter.

Glad you have an independent check with your NaI counter. But one would expect a strong correlation if it were fusion, e.g. one Watt should give a thousand billion neutrons per second - surely a strong correlation!

Dick Garwin's address is

RLG2@YKTVMV

and he is very good at offering useful comments.

Best Wishes,

Douglas.

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Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 0582; Mon,
05 Feb 90 14:37:47 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Mon, 05 Feb 90 14:37:43 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA14662; Mon, 5 Feb 90 14:35:46 +0100
Message-Id: <9002051335.AA14662@dxmint.cern.ch>
Date: Mon, 5 Feb 90 14:37 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Letters from Droege t Fermilab.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear Dick, 5 February 1990.
Here are three earlier messages that may help to explain the
later ones.
Best Wishes,
Douglas.

From:
VXCERN::VXGIFT::MINT::"DROEGE@FNALC.decnet.cern.ch" 7-NOV-1989
22:09:48.61
To:
dromcd@vxcern.decnet.cern.ch
CC:
Subj:
"Cold Fusion a.k.a. Anomalous Heat"

Date: Tue, 7 Nov 89 14:16 CST
Message-Id: <8911072113.AA19393@mint.cern>
X-Original-To: dromcd@vxcern.decnet.cern.ch, DROEGE

Thank you for all your work in preparing COLD FUSION NEWS. The
notes have been most helpful. Please put me on your list.

Since June I have been running a "Type C" calorimeter experiment
in my basement. The work is not supported by Fermilab.

The calorimeter is a nul balance type of my own design. Present
sensitivity and stability allow calibration to about 5 mw RMS.
The test cell is sealed with a catalyst. A sensitive gas system
monitors gas balance.

"Anomalous Heat" is seen at the level of 60 mw (2.5 w/cm³) after
charging over runs of 7 days. An energy balance is kept from the
start of each run. A typical (most recent) run accumulated 29443
Joules over seven days.

I measure excess gas in the closed system with a wonderful "Rube
Goldberg" arrangement using a horse syringe, a stepping motor,
a very sensitive pressure switch, and some fancy electronics. I
presume (my resources are limited so I cannot do everything I
would like to do, like send out a gas sample for analysis) that
the excess gas seen during charge is oxygen from the D going into
the sample. I do not yet make barometric corrections.

I periodically measure the impedance of the sample with a four terminal arrangement at pulsed currents of 1,10,100 ma and 1 amp.

1) I have seen as much as 105 cc excess gas during a 7 day charge cycle at .6 amp per sq. cm. This would indicate 210 cc of D in the 0.0229 cc Pd sample.

2) The impedance starts out consistent with the known resistivity. It increases over the first few days, per published data. After a few more days, it starts to decrease, eventually the measurements are consistent with a highly ionized object. My measurements are similar to Moore's in a 1939 Princeton dissertation, except he used very fine wires and reached 2800/1 volume ratios. He found that more of the gas (hydrogen) went into "rifts in the slip planes" then went into the lattice.

3) On turning off the current, the gas gradually comes out, with a several day time constant. Heat measurements during this time are consistent with the recombination of the gas in the catalyst. This gives some confidence that the calorimeter is working correctly since charging shows a net loss of 10 mw and discharge shows a net gain of 20 mw. This is consistent with an estimated calorimeter drift of 5 mw.

I make no claims about fusion. All attempts to correlate counts in a 1" NaI counter with anything going on in the experiment have failed. This includes a test where the cell current was alternated between a high and low value and counts were measured at low current, low to high transition, high current, and high to low transition. During this test about 3 cc of gas pumped in and out of the sample and "anomalous heat" at the level of 100 mw was seen in the 0.0229 cc sample. Electrolyte samples sent to a commercial laboratory for tritium analysis were consistent with background. No correlations have been found with the anomalous heat which varies over a 4/1 range.

Whatever else that can be said, this experiment is great fun. (translate=damn hard). There is no end of new ideas to explore. As P&F say "this experiment raises more questions than it answers". It has already had a spin off in that a neighbor looking at my calorimeter noted that if I could make the heat measurements that he say, then he had a commercial application. We now have a test instrument operating which shows great promise.

Thomas F. Droege, DECNET CDFPIG::DROEGE OR FNAL::DROEGE

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Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 0604; Mon,
05 Feb 90 14:38:41 GVA

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Mon, 05 Feb 90 14:38:38 GVA

Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA14681; Mon, 5 Feb 90 14:36:49 +0100

Message-Id: <9002051336.AA14681@dxmint.cern.ch>

Date: Mon, 5 Feb 90 14:38 GMT +1

From: MORRISON%VXPRIX.decnet.CERN@cernvax

Subject: Letters from Droege t Fermilab.

To: rlg2@yktvmv

X-Vms-To: MINT: "rlg2@yktvmv"

From:

43028::DROEGE 11-NOV-1989 00:37:42.63

To:

22719::MORRISON,FNAL::RAPIDIS,DROEGE

CC:

Subj:

Calorimeter Details

Here are some details of my calorimeter. The device consists of two copper shells, each covered with 1 1/2" of styrofoam. The inner shell is connected to the outer shell through a thermoelectric cooler (Peltier). The outer shell is connected to a water cooled heat sink through a pair of thermoelectric coolers. The outer shell coolers are operated by a power operational amplifier to maintain a constant temperature. Typical range is +/- 0.05 degrees K from the set point (310 degrees K) after turn on transients.

The thermoelectric cooler for the inner shell is driven by a constant current power operational amplifier. The sensitive electronics for this amplifier and the current shunt are located in the temperature controlled outer shell. Calibrations indicate that the cooler pumps a constant amount of heat if operated at constant current with both sides at the same temperature.

A distributed precision resistor glued to the inner wall of the inner shell is driven by a third power servo amplifier to maintain the inner shell at the same temperature as the outer shell. The thermal path between the inner and outer shell (almost entirely through the thermoelectric cooler) is relatively high impedance compared to the thermal capacity of the inner shell and test load, so the inner shell integrates any power put into it with a long time constant. Since the inner and outer shell are maintained at the same temperature, the calibration process is simplified.

By placing a dummy load in the inner shell and driving it from a fourth power amplifier, a series of calibrations can be performed. As power is put into the dummy load, the balance servo removes power from the precision resistor. The only problem is the large thermal time constant (1 1/2 hours) which makes the calibration tedious. When the dummy is replaced with an active cell the same voltage and current measurement channels are used.

Corrections are made for the variations in temperature between the inner and outer shell. The variable term is of order 0.015 degrees K RMS. The correction has a relatively large constant term, 300 mw when operated at 6 W total power, but the variable term is seldom larger than 5 mw. The constant term has changed only a few mw since operation started. The constant correction term is primarily due to the two thermometers (AD590) not reading the same voltage at the same temperature.

Energy in and out of the cell is measured by a high speed data system, and means of 100 samples are written to disk every thirty seconds. Comparison of mean and RMS cell E*I measurements give us some confidence that we measure power correctly. Still, this is a weak point and will be the subject of refinement before any publication. This and every other servo system I have ever built has oscillated. But no longer. I bet Commonwealth Edison would like to hire Garwin to compute electric bills.

Calibrations to date have repeated over several months within 25 mw. I have reason to believe it is really much better. I keep learning. It is easy to detect 1-2 mw from a separate test resistor. I have learned a lot about commercial Peltier coolers.

Comment on note #20 part 2, section 5.2:

Whatever is said about "anomalous heat", my present 1.27 mm dia. sample appears to be doing interesting things (like absorbing D, and changing resistivity) after 7 days. This is supported by Moore's thesis where curves taken on fine wire (I remember 0.001") are still changing after many hours. I tend to support the P&F charge time of $(8 \text{ days}) \cdot (r \text{ in mm})^{-2}$ which I compute from their various statements. This suggests your requested 1 cm rod could still be active after 200 days. You may have to wait a while for someone with enough patience to perform such a run. Note that the Texas A&M results are with 0.5 mm dia rods which should charge in about 1/2 day per the above estimate. One advantage of running a closed gas system and of measuring the sample impedance is that there is some action in an otherwise tedious experiment.

Please note that after 7 months of lost sleep I do not have a single repeatable result. Every run has ended in some sort of disaster. Still, I see enough to keep working. I am beginning to think of rebuilding the calorimeter a third time using Jupiter probe engineering techniques - instead of the string and glue I used when it looked like there was a race against time.

Tom Droege

qq
&EXIT

THIS FILE HAS BEEN RECEIVED FROM BITNET

The file may be executable. Before removing this header you must understand what the code will do. You must also have the appropriate intellectual property agreements in place before receiving the code into IBM.

If you have any questions, contact your manager.

The contents of the file has been shifted right by one character.

Filename=(none) Filetype=(none) RECFM=F LRECL=80 Records=92

The file received from the BITNET gateway begins below the next line.

Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 0636; Mon,
05 Feb 90 14:39:43 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Mon, 05 Feb 90 14:39:40 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA14698; Mon, 5 Feb 90 14:37:50 +0100
Message-Id: <9002051337.AA14698@dxmint.cern.ch>
Date: Mon, 5 Feb 90 14:39 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Letters from Droege at Fermilab.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

From:
43028::DROEGE 22-NOV-1989 01:14:47.67
To:
22719::MORRISON,FNAL::RAPIDIS
CC:
DROEGE
Subj:
Garwin's explanation of "anomalous heat"

Grasping at Straws - Possible Explanation of Excess Heat

We have now tested our calorimeter for the Garwin effect.

Our test takes sl. The voltage and current measurements are taken 10 milliseconds apart. The data system does not allow simultaneous measurement. 100 measurements are taken and the instantaneous products are averaged and compared to the product of the mean voltage and current.

Result: Product of the mean voltage and current equals the mean of the instantaneous products to one part in 10^{-6} . Much better than I would have expected from a 12 bit system but that is the result. Largest ratio from 6000 tests performed over 50 hours of running was one part in 10^{-5} - an isolated spike. Tests on noise gave 2% effects.

Note that use of either a constant current or constant voltage power supply guarantees the above result by theory. It is hard not to use one or the other. There only remains bandwidth arguments. Sampling at rates much higher than the component

bandwidth as we do should convince most.

We operate at constant current. We chose this mode because a little knowledge of electroplating cells led us to realize that current density was likely to be the correct parameter to control. We designed our own circuit and it oscillated. Being quite familiar with the symptoms of oscillating circuits (I admit they are not that dissimilar from the reports of excess heat) we quickly fixed the problems and have measured the stability at better than one part in 10,000.

While it is likely that somewhere, someone momentarily confused an oscillating power supply for excess heat; it really offends me to see how wild speculations are taken as "proof" that no phenomena exists. I have spent some time trying to design a "bad" power measurement set up which would duplicate the published results. It requires an incredible combination of long term stable oscillating power supplies, just the right amplitude of oscillation, and bad selection of voltmeters and ammeters. Experimenters using widely differing technique, for example Appleby and Huggins, at widely separated locations have to hit upon similar bad technique. Early press visits to Pons and Fleischmann's lab noted storage batteries. Try to make one of these oscillate!

Naturally I have been dragging my colleagues by my apparatus. They typically give the matter 30 seconds thought and then announce that I am doing "x" wrong. Doing "x" by their method usually requires about 2 weeks work. A recent example will illustrate. Since my calorimeter integrates the power put into it, I had not paid enough attention to the form of the calibration resistor I substituted for the cell. The argument was made that there were somehow different temperature gradients in the chamber with my somewhat differently shaped calibration resistor. So I built a cell identical to the electrolytic cell and have spent the last two weeks in calibration. There was no change in result. Now I realize that since I filled my test cell with oil to prevent corrosion that the next critic will want me to use water!

My plea is for more thoughtful criticism. Garwin assumes we are all sophomores. He has caused me a lot more work than he spent dreaming up his unlikely idea. Yet Garwin is a "great man" and the rest of you mindlessly nod your heads and say "yes, he has the explanation."

Again, I remind you that I am not a "believer". This is a great exercise in careful measurement. It is an opportunity to solve new problems. I am not even sure I will be able to keep working as my neighbor wants us to go into business with the spin off and make big bucks!

Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 7411; Sun,
04 Feb 90 13:29:28 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Sun, 04 Feb 90 13:29:27 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA01830; Sun, 4 Feb 90 13:27:43 +0100
Message-Id: <9002041227.AA01830@dxmint.cern.ch>
Date: Sun, 4 Feb 90 13:29 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Message from Tom Droege about Cold Fusion
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

From:
43028::DROEGE 11-JAN-1990 00:26:04.04
To:
22719::MORRISON
CC:
DROEGE
Subj:
Status of our anomalous heat experiment.

More news on "anomalous heat"

0) This paragraph is designed to self destruct sio I know it can
be done right, but just now I
have things (like Chemistry) that I would rather learn than VAX
manipulations.

0) This paragraph is designed to self destruct during the process
of transmission. I know it can be done right, but just now I
have things (like Chemistry) that I would rather learn than VAX
manipulations.

1) Sorry to keep bothering you, but I need a neutral third party
to listen to what I am doing and to possibly give me a kick if I
am doing something stupid. My friends want me to see something
and tend to cheer everything I do. My physicist friends mostly
think I am crazy and act like they don't know how to tell me so.
So I could use serious criticism.

2) My cell was neutralized by LiHCO_3 or Li_2CO_3 not sure which
yet. My chemist friend also thinks there is something else. The
diffusion of CO_2 into the system and the formation of carbonates
looks a little small to explain anomalous heat. It is tempting
to try to use this to explain the Texas A&M Lithium vs Sodium
result since Lithium has a larger heat of formation.

3) P&F mention sealing their cell with "Parafilm" but do not say
how they trap the outgoing gas. Gur at Stanford says he uses a
bubbler on their closed system. The Texas A&M papers do not
mention seals, but they do mention adding electrolyte, and
Appleby in his Santa Fe presentation said that the cells were not
tightly sealed. I have not heard anyone discuss diffusion of CO_2
into the system, but a number of people have mentioned the
increase in cell voltage with time.

4) It takes about 5 days of operation for my cell to go from Ph of 13 to 10.5 probably from diffusion of CO₂ through the water trap. There is a small possibility that current through the cell is required for the loss of Ph. Every test in this experiment takes forever. I have replaced the water trap with sodium hydroxide ("Easy Off" oven cleaner). Since I use plastic (first Polystyrene, now Polypropylene) cells it is possible that the LiOH is grabbing carbon from the plastic, particularly when there is nice fresh H and O around and all that catalyst.

5) On the Garwin effect, I have now seen ratios as small as .994 with very active cells. But it is always in the direction of reducing the apparent excess heat. One more thing to check out.

6) After being very friendly on the first two calls, Murphy at Texas A&M has not returned a call since I mentioned Ph. The secretary says he will be too busy in the lab for a few days.

7) We are running Pd-Pt in D₂O, Pd-Pt in H₂O, Pt-Pt in H₂O, and Resistors in Oil. Everything is fine at the 1% level but nothing makes sense at 0.1%. Yet each one is repeatable to 0.05% long term drift. So I can not be very certain about my 2% effects. Unfortunately I did not build everything into my calorimeter that is needed. It's string and glue construction does not really allow change. So I am trying to decide if it is worth the effort to build a really good second generation device.

8) I believe that I could now build a 0.01% calorimeter with 16 bit instrumentation, and room for a 500 cc 25 watt experiment. I have bought some of the pieces, and done a lot of thinking about how to do it right. Does anyone care?

9) I have gone from "pretty sure" to "neutral" on measurement of anomalous heat.

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id AA01838; Sun, 4 Feb 90 13:28:29 +0100

Message-Id: <9002041228.AA01838@dxmint.cern.ch>

Date: Sun, 4 Feb 90 13:30 GMT +1

From: MORRISON%VXPRIX.decnet.CERN@cernvax

Subject: Message from Tom Droege about Cold Fusion

To: rlg2@yktvmv

X-Vms-To: MINT: "rlg2@yktvmv"

From:

43028::DROEGE 18-JAN-1990 20:22:44.03

To:

22719::MORRISON

CC:

DROEGE

Subj:

"Anomalous heat"

1) I am back to "pretty sure" from "neutral"

2) I have found several problems with my calibration scheme and now get
consistent results at the 0.1% level.

3) Presently seeing about 60mw out of 6w "anomalous heat".

4) Since you have reported many site visits where the cell did not
happen to be working at the time, here is your opportunity to visit
an experiment where "anomalous heat" is claimed to be happening.

5) I warn you that it is about as exciting as watching paint fade.

Tom Droege

=====

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Message-Id: <9002041228.AA01834@dxmint.cern.ch>
Date: Sun, 4 Feb 90 13:29 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Message from Tom Droege about Cold Fusion
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

From:
43028::DROEGE 16-JAN-1990 19:28:59.00
To:
22719::MORRISON
CC:
DROEGE
Subj:
Your Visit to Fermilab

Sorry to be so late with the offer, but I would be happy to put you up while at Fermilab if you have not made other arrangements. My extension at Fermilab is 3286. My home phone is 879-7609, and there is also 879-2949 which answers "Environmental Optics" and has an answering machine. I could also pick you up or take you to the airport.

After cleaning up the Lithium Carbonate problem, I still seem to see excess heat. It is small, but still 10x my probable calorimeter drift. Anyone seeing excess heat, particularly with LiOH, should examine whether CO2 can get to their electrolyte.

I would like to invite you to visit my experiment. I live in Batavia, which is just west of Fermilab. We could get there in about the same time it takes to drive across the lab. Anytime, including all weekend would be fine.

Tom

 Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 7422; Sun,
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id AA01845; Sun, 4 Feb 90 13:29:00 +0100
 Message-Id: <9002041229.AA01845@dxmint.cern.ch>
 Date: Sun, 4 Feb 90 13:30 GMT +1
 From: MORRISON%VXPRIX.decnet.CERN@cernvax
 Subject: Message from Tom Droege about Cold Fusion
 To: rlg2@yktvmv
 X-Vms-To: MINT::"rlg2@yktvmv"

Dear Tom,

24 january 1990.

Thanks for your messages which I got on my return to CERN.
 Unfortunately I did not get to Fermilab. I had not known that Monday 16 Jan
 was a holiday Martin Luther King day which upset my schedule, on the other
 hand it did allow me to ski at Alta whereas it is very difficult to find snow
 in Europe. Your offers were very kind, sorry I missed out.

Let me try and respond to some of your questions/comments;

1. "My friends want me to see something and tend to cheer everything I do "
 This sounds dangerous.

"my physicist friends". the problem for physicists is that they
 look at ALL the evidence and not bits selected here and there. That is they
 make judgements. Further they understand that one cannot easily accept some
 local changes to the laws of physics without requiring changes elsewhere,
 often of results very well established. For example, the ratio of neutron
 to tritium production in d + d reactions is very well established to be one and
 this is in agreement with some very general properties that have been tested
 extensively in the required range. Hence when some people claim ratios of
 10 to the power -8, well it is really easier to believe they are making some
 mistake and the fact that many big carefully done experiments, e.g at Harwell,
 find no neutrons and no tritium, then this goes in the same direction.

2. The question of diffusion of CO2 into your cell, is a new idea to me.

Normally the hole through which the D2 and O2 gases escape is small so that
 little can come in to the cell. But suppose it were a factor, then there are
 two questions, (1) the extra heat would be of chemical origin and most
 chemists say that chemical energies are too small to account for the effects.
 On the other hand if this is a factor that has been neglected, maybe it
 would give some heat for a time and if the factor to multiply up were large
 enough it could then appear to be important

(2) If CO2 were important, then would one expect any difference between H2O
 and D2O ? What do you find for H2O ?

3. See above. Most groups let the D2 and O2 gases escape and add D2O daily -
 was told that at the Natl. Cold Fusion Inst. one needs 1 1/2 litres of D2O
 to replenish, so they are thinking of recombining the gases externally with
 a catalyst(which could be Pd or Pt, initially heated to about 150 degrees).
 However as most people agree, it is much better to use a completely closed
 system with a catalyst in the cell - this makes the corrections using
 Newton's law of cooling unnecessary. Was told at the Institute that they had
 just had a visit from the GE experts on heat transfer who said the errors
 on these calibrations of cooling losses were difficult and were about 10
 to 15%.

P & F do not trap the outgoing gases.

4. as you say it is surprising that people do not measure Ph.
7. Fine at 1% level (definition of "fine" please). It sounds as if you are
 getting reproducible results at the 1% level but not at the 0.1% level.

Maybe as I hinted in 3 above, there are systematic errors and uncertainties at the levels higher than the 0.05% error you claim. Am not sure what you are describing when you say nothing makes sense at the 0.1% level yet is repeatable at the 0.05% level?

8. We are in a strange situation now. I have described three phases in which in the third phase there is an avalanche of negative results. But now we seem to be entering a fourth phase where most results are positive. The explanation may be that most careful experimenters have done their experiments by now and found no effect, so they have stopped as they have no improvements to make whereas those who have found positive results all have non-reproducible effects and so have an urge to continue and understand. Does anyone care - yes, and there is a forum to report it at the NCFRI meeting at the end of March (there is a good chance that I will be there). It is always more satisfying to finish a subject by doing an improved experiment. May I suggest that if you are having fun, then you should continue. Edward Teller told me that he does not believe that there is any evidence for cold fusion, but he wants to have fun so he has tried to invent a particle which would have properties that which would explain anomalous results.

Note of 18 January

You talk of 60 mW out of 6 Watts in, while the background is stable to 0.1% - sounds like a ten standard deviation effect - but would like to see the results. Also what do you get when you run normal H₂O? In this business tests and calibrations are very important.

If you are observing 60 mW, then if this were fusion there should be about ten million million neutrons per second - a lethal dose!

Some people only mention the heat spikes they observe during the running. It is better to measure the power (heat excess) continuously from the moment of switching on as it is possible that a lot of energy is put in, to expand the palladium lattice etc., so that if one later gets an excess heat spike, this may be the energy put in earlier coming out.

Will be interested to hear how you are getting on.

Best Wishes,

Douglas.

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Date: Sun, 4 Feb 90 13:31 GMT +1

From: MORRISON%VXPRIX.decnet.CERN@cernvax

Subject: Message from Tom Droege about Cold Fusion

To: rlg2@yktvmv

X-Vms-To: MINT:."rlg2@yktvmv"

MESSAGE FOR DAVID WILLIAMS

Dear David,

30 January 1990.

After talking to you on the phone this afternoon, I received the message below from Tom Droege in Fermilab. Am told he is a very sharp guy who is a good experimenter. He decided to test Cold Fusion and has been doing some experiments in his basement. He has been writing to me off and on from some time, sometimes claiming to see excess heat sometimes not. He has been measuring the Ph of his cells (something he thought all chemists did but when he asked people of Texas A&M, they stopped returning his phone calls!) and finds it varies - he suspects CO2 is getting in and giving carbonates and heat. He has been invited to the March Meeting at the Cold Fusion Institute.

Do you have any comments? - you could contact him directly at

Droege@fnal

or I will pass things on for you,

Best Wishes,

Douglas.

From:

43028::DROEGE 30-JAN-1990 00:33:35.60

To:

22719::MORRISON

CC:

DROEGE

Subj:

Preliminary results on effects of CO2

Some day I will learn to send files. Until I do, these immortal words will be sacrificed to the VAX god. I bid him good eating.

Some day I w VAX god. I b
id him good eating.

Problems with CO2

We have been operating closed systems for some months. For the first run described below, the cell was vented into a piston whose motion is controlled by a sensitive pressure switch (< 0.05 inches of H2O) and a stepping motor. To protect against explosion when I am not around, there was another path to the atmosphere through a water trap.

First let me outline the general run procedure. A cell is put

into the calorimeter and the operating temperature set. The cell is operated at zero current during this time.

When the temperature has stabilized, which takes about 8 hours, a data run is started. Data is taken at zero current for another period to establish calorimeter drift. The cell is then operated at current for a period, then it is returned to zero current. We keep an energy balance during the entire run. For cells which are not supposed to give off excess heat, we expect the balance to come out to zero at the end of such a run. Any difference is the calorimeter drift.

The following run descriptions are the best I have to implicate CO₂ diffusion. There is also a very crude chemical analysis done by a chemist friend, Rick Mouche, which detected carbonates (and something else), and a change of pH from 13 to 7-8.

a) Run P1005 used a Pt-Pt cell with 1N LiOH. The run started with operation at zero current for 200 minutes. During this time the difference between energy removed and heater energy was -100 J. The refrigerator was set for 8.031 watts. This means we put 96,732 J into the calorimeter with the balancing resistor, and took out 96,832 J through the refrigerator. This is typical of the balance that we have been able to achieve. With much longer calibration periods and great care this can be improved about an order of magnitude.

The current was then set for .72 amps for 629 minutes. during this time -3250 J were accumulated. There was a net gas evolution of 10cc although this is not reliable as the cell sprung a leak near the end of the run. It is reasonably safe to assume that most of the evolved gas was recombining in the cell since the evolution rate is about 15 cc/minute at this current.

The cell was then run an additional 221 minutes and returned to a near balanced condition, but with the above energy accumulation.

b) Run O1007 was now performed using an oil filled resistor cell. The cell was operated at currents of 0,.5,.25,.05, and 0 amperes over 2252 minutes. The energy balance returned within 750 J out of a total run energy of 1.08 MJ.

c) The water trap was now replaced with one containing NaOH ("Easy Off" oven cleaner - this is a basement experiment).

d) Run P1007 was now made using the same cell as in a) at an operating current of 0.84 amperes. This run balanced within 250 J over an 840 minute run with 0.4 MJ through the calorimeter.

I have looked at a number of old H₂O runs, which now make sense if I assume that CO₂ was producing heat. These were done with .1N H₂O and .1N D₂O. Both show excess heat, but the D₂O runs show more heat than the H₂O runs. The H₂O runs show less than the 120 mw rate of run a) above, presumably because run a) had more LiOH to convert.

I have several long D₂O runs which were made with the NaOH trap. These runs displayed "anomalous heat" and the pH dropped only to 12 from 12.5 at the start of the run. I have now started a series of runs with D₂O and H₂O cells with a completely sealed system.

The runs above were not planned for the confirmation of the CO₂ diffusion so it is not a consistent set. We will do follow up runs when we can find the time. This work is better left to the chemists who know what they are doing.

Preliminary conclusions:

Something changes the pH of cells which are even slightly open to the atmosphere. The likely cause is CO₂ which diffuses easily through a water trap. This forms carbonates with the liberation of energy. Use of NaOH in the water trap greatly reduced the "anomalous heat". Whatever is happening is at least accelerated by cell current, as little "anomalous heat" appears without it.

Discussion:

All cells when operated with a water trap have shown losses of pH. I have saved electrolytes from all runs. Checks of these indicate that most ended up near 7 from a starting pH of near 14 as measured with pH paper. In general after a few hundred hours I was unable to drive enough voltage to the cell to maintain desired current. Previously I thought this was "electrode passivation" (electrochemists have their own language) and discarded the cell. Recently, after noting the pH drop, I have been able to revive cells by replacing the electrolyte.

While Rick says that the cell he tested had come to some equilibrium condition of LiHCO₃ and Li₂CO₃, he believes that he saw something else. Possibly a nitrogen compound. All efforts to get him to take a sample to NALCO (a water treatment company which has all the latest analysis stuff) where he works have failed. So I do not have access to a serious chemist. While I would be quit willing to pay for an analysis, I do not even know the right words to say to a commercial lab, and have not yet persuaded Rick that he wants to take on the project. So I am temporarily stymied.

I note a small black cloud in the NaOH trap. Can there be some Li compound coming off? Lithium oxide is apparently black. My brother Lee notes that Lithium quickly turns black on exposure to air.

All the arguments that I have heard relate electron volts released to atoms of Pd. Observers note a few thousand electron volts per atom of Pd and say this cannot be chemistry. But what if we are doing something wonderful to water - like making "brown water", "poly-water" (sorry I am not a completely serious person and cannot resist a joke, even though this effort seems to have lost its sense of humor) or even some energetic nitrogen compound? You say NCFI uses 1.5 litre per run. This is enough to hide a lot of chemistry. Are they sure nothing is coming off in the water vapor?

I used to notice that the runs gave out more heat when I was in the room. I kept wrapping more insulating blankets around the apparatus. Could it be that it saw my CO₂?

Still, I must report that my evidence favors excess heat now that I am operating a sealed cell.

I am now very suspicious of the Texas A&M Lithium/Sodium experiment since the result is in the right direction for chemistry. I would want to see the same experiment run completely sealed, and with careful pre and post analysis of the electrolyte.

I will send more details on calibration procedures when I can get them written up. The above discovery, and finding some lead resistance that I had neglected in my cell now give me consistent calibrations at near the 0.1% level.

Tom

New Energy Times Archive

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Message-Id: <9002041229.AA01861@dxmint.cern.ch>

Date: Sun, 4 Feb 90 13:31 GMT +1

From: MORRISON%VXPRIX.decnet.CERN@cernvax

Subject: Message from Tom Droege about Cold Fusion

To: rlg2@yktvmv

X-Vms-To: MINT::"rlg2@yktvmv"

From:

43028::DROEGE 1-FEB-1990 01:04:24.01

To:

22719::MORRISON

CC:

DROEGE

Subj:

Anomalous Heat

More news on "anomalous heat"

I know

it can be done right, but just now I
have things (like Chemistry) that I would rather learn than VAX
manipulations.

Dear Douglas,

Thank you for taking the time to read my comments. I know you
are busy and greatly appreciate having a place to communicate.

This will cover:

1. Comments on your message.
2. Calibration procedure.
3. Current status of the experiment.
4. More on the Garwin effect.

1. Comments on your message.

"my physicist friends....." The problem is not that they look at
all the evidence, I would welcome that. The problem
is that a suprising number have closed their minds and will not
look at any evidence. I have done thirty years of hard work for
physicist friends, much above and beyond the call of duty. I
have tried to call in some of the "debt" on this experiment. A
few have listened a little. The trouble is, I know mostly senior
people who are pretty smart. Once they have looked a little at
what I am doing, they quickly realize that to understand the
experiment will take a lot of hard work. Then the excuses come.
So far I have only found one young post doc that is looking at my
evidence and asking serious questions. Fortunately, I always get
help when I ask a specific question. But my position is weak if
I must think of the right thing to ask.

Referring to previous notes #7 and your Jan 24 message, fine at the 1% level meant a calibration run with say an oil cell and a Pt-Pt in LiOH cell closed back within 1% of the total number of Joules passed through the cells with the same calibration constants but required different calibration to match to .1%. At the time I wrote the last note I was assuming that there was no net power from the Pt-Pt cell since there was little net gas. Now I know that the Pt-Pt cell was producing heat, and have a run which shows net power of 86 mw before closing the cell and 5 mw +/- 20 mw after. (Statements of error I make in these notes generally mean the largest error I can imagine based on the few runs I have. While 100 runs and an analysis would be nice, I won't live long enough to make them. I could really use some help on analysis of the isolated type of runs that these experiments produce.) Repeatable at the 0.05% level means that I can take a cell off the shelf, put it in the calorimeter, and have the total number of Joules into the cell balance the total number of Joules out of the cell within 0.05% using the same calibration constants for two runs performed days apart (for an oil cell).

Stage 4. I take the same umbrage to your statement "The explanation may be that most careful experimenters ..." that you took to the gentleman at your lecture at Utah. It seems to me that among the class of all experimenters attempting this work there will be a mixture of talents. Since there is a broad range of experiments to be tried, some will pick experiments which give clear negative results. Some of this group will do the experiment well and will publish and leave the field. Some will do the negative experiment poorly and may keep working. Some will pick an experiment for which it is relatively easy to get positive results. These experiments may have a hidden trick that is hard to discover. So this group will tend to keep working. So far, I have learned a lot of new things. Certainly a lot about heat. I consider my time well spent. I still see "anomalous heat" and still don't know the trick.

I don't think it is so easy to separate careful experimenters from bad ones. The luck of experimental choice has a lot to do with it. Since I was sick of building radiation measuring instrumentation I went for heat for a change of pace. (I also thought it was the right thing to examine). Others, like Gai, stuck to their experience.

I understand the problems that a non-radiation producing experiment would pose for our understanding of Physics. It is precisely this long shot that keeps me working. I would probably not make this choice if I were young and trying to make a reputation. One of the advantages of being (almost) 60 is that I can take a big chance with no serious consequences if I have guessed wrong. It is not only the young that get to be daring!

2. Calibration procedure.

The following is a discussion of the calorimeter calibration procedure. It is probably too long for you to read and was done mostly for my own benefit. A scan will give you some flavor of my technique. You may want to refer to the earlier message which described the calorimeter.

I have really struggled with the calorimeter calibration. It

looks like problems were mostly due to CO2. I try to keep track of total enthalpy from start to the end of a run.

I first bring the cell up to the operating temperature of the calorimeter. Then a run is started. I keep an energy balance throughout the run. Usually I will run a day at zero current, then step through several cell currents, then back to zero current. Starting with a zero sum energy balance, the cell balance is measured at zero cell current. A small variation here is assumed to be the drift limit for the run. When stepped to higher current, there is a net flow of heat into the cell. This is because there is a redistribution of heat in the cell, more heat is in oil(water) and less in the copper. When stepped to a lower cell current this heat is recovered. At the run end when the cell is returned to zero current, the energy balance returns to zero within some error, or there is "anomalous heat".

With the oil cell, the whole process is quite repeatable. It is easy to keep the balance to 0.05% or 1500 J out of 3 MJ put into and taken from the calorimeter for a typical 100 hour run. Some runs have done one part in 10,000. I have not reached the limit of what the calorimeter can do, only the limit of my patience in calibration. The cell can be removed and replaced and the same calibration constants used. The critical time constant is 1.5 hours so measurement points tend to take a day each.

There are a number of calibration constants. First there is the resistance of the cell heater. The goal of calibration is to reference everything to the cell heater so that even if an error is made in the heater value, a relative calibration will be obtained which is sensitive to the calorimeter heat balance. The heater resistance is measured with a 5 Digit meter which claims 0.018% accuracy. In situ tests indicate that the heater could change as much as 0.06% due to load changes as power is shifted between the cell and the heater. The temperature coefficient has been measured at 16ppm/degree C at the operating temperature.

The next constant is the thermo-electric cooler (TEC) efficiency. For accuracy we always run the TEC at constant current and constant temperature. We use the same current and temperature for a series of runs. The current is derived from a precision reference using high stability operational amplifiers operated at constant temperature. This electronics and the cooler shunt are located in the outer shell of the calorimeter which is typically held to 0.02 degrees kelvin. To calibrate, we select a convenient TEC current. The calorimeter is allowed to balance and the heater power noted. Through a very low offset multiplexer, the resistor voltage and TEC current are read into the same ADC. This determines a cooler heat removal constant that is relative to the balancing resistor. Ground and a reference voltage are also read through the multiplexer to verify the integrity of the ADC.

There is concern that the TEC constant will change over time. In fact I had this problem in all the early runs until I learned that it is easy to damage them by mechanical stress. The cooling efficiency always goes down if they are stressed so it is relatively easy to be en careful and have been able to keep the same constant for the last two months. The next calorimeter design will provide for a stress free mount.

Next we install a cell containing resistors in oil which has the same configuration as an electrolytic cell. This cell is run and stepped through several currents so that EI of the cell matches $E \cdot E \cdot R$ of the balancing resistor. This produces a correction for the cell current shunt which may not be absolutely accurate but is relatively accurate compared to the R of the heater.

Note that this procedure makes sense for someone operating at home without access to a standards laboratory. The meter that I can afford at home does a pretty good job of measuring resistance. I don't really care about absolute value, just the number of digits and linearity. An error in absolute resistance value produces roughly the same error in "anomalous heat", but not of total calorimeter heat flow, which is much larger. I have also run tests that show the resistor is well behaved when operating under power. This has been done by running the resistor at full power, the quickly switching off the drive and watching the resistance change as it cools down. If this were an official Fermilab project I would take my shunt to the instrument shop for calibration (then I would also perform the relative calibration).

The next correction is for the thermal path between the inner and outer shell. If the two are not at the same temperature then there is heat flow. While the inner shell is well insulated with foam, there is a high conduction path through the TEC to the outer shell. If a correction is derived from experiment and depends on inner shell temperature and the difference between inner and outer shell temperature. This correction is small, and constant except during transient cell power changes.

We measure all voltages differentially. We planned this to prevent problems with lead losses, ground currents, etc.. Unfortunately until recently we measured the voltage directly across the cell and heater. We did this to avoid errors due to connector voltage drops. Recently I realized that this was a small heat source in the calorimeter. So with two additional corrections for the leads in the calorimeter to the cell and heater, and sealed cells so that carbonate formation does not occur, I am now able to obtain 0.1% balances between different cells.

There are a number of problems in the present design. The whole system, including the computer data acquisition, was put together in about two weeks. I did not think about it as much as I now have. The present configuration contains thermal gradients in both the inner and outer cell. Fortunately, they are in the same direction and tend to cancel. Still I know better now how to design such a device. The next generation, if I build it, will be inside a dewar.

3. Current status of the experiment.

I have now seen 300 mw peak over one minute, 200 mw peak over a ten minute weighted average, and 140 mw peak over a 100 minute weighted average in a sealed D2O cell. A conservative estimate of the calorimeter drift is 20 mw. The values nearly doubled over a 60 hour run. Two of my other measurements also increase over this time, the absorbed gas and the cell voltage. If I am somehow not adding up power correctly, then the cell voltage

change would explain the power. If this is OK, then the gas measurement is very intriguing. Now running a sealed H2O cell. Stay tuned.

4. The Garwin effect.

Since I was critical of chemists not using their pH meter, I thought I had better use my oscilloscope. So I looked at the cell voltage. What one sees, is a saw tooth wave form in voltage when a cell is run in constant current mode. If run at constant voltage, then a saw tooth current results. The explanation is that as the bubbles grow, they reduce the electrode area and the cell resistance goes up. They then break away quickly which causes the fast drop at the end of the saw tooth. When operated at 500 ma per sq. cm my cell displays a run up of from the 8 volt level to 10 volts in 2 seconds followed by a 50 ms drop back to 8 volts. While somewhat random, it is surprisingly regular. It is as if the bubbles always grow in the same place. At lower current densities the run up is proportionately longer.

Such a wave form is easy to measure incorrectly. Workers should be cautioned, particularly the chemists who presumably do not think in terms of pointing vectors. For example, a successive approximation ADC will almost always measure such a signal too low, even converting at high speed. A normal digital voltmeter will usually have a terrible time and just read nonsense as it is designed to reject 60 cycle and lets low frequencies through. A fast sample and hold is called for along with a procedure which computes a correct effective value.

Regards,

Tom

Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 7431; Sun,
04 Feb 90 13:32:17 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Sun, 04 Feb 90 13:32:15 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA01868; Sun, 4 Feb 90 13:30:30 +0100
Message-Id: <9002041230.AA01868@dxmint.cern.ch>
Date: Sun, 4 Feb 90 13:32 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Message from Tom Droege about Cold Fusion
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear Tom,

2 February 1990.

Thanks for your long letter of 1st February.

1. "physicists friends" I sympathise with you and with them. For example just now I am spending some time trying to help Roumanian physicists who have suffered badly but would like to start again to do research. But I ask myself why Roumania and not Hungary or ...or... Guess the answer is there are many deserving causes and one devotes some time where one feels in a special position to help usefully. With Cold Fusion most physicists in CERN are convinced that there is nothing in it and hence devoting a lot of time to it is not very profitable. It is possible that it is the same in Fermilab. Here people are happy to get the latest news on Cold Fusion, but that is all. There is one very good physicist, who is American, who could advise you in detail as he has helped others, and that is Dick Garwin - I will contact him for you.

1A. Errors are a difficult subject but very important. It sounds as if your quoted errors are 1 to 2 standard deviations depending on the number of trials you have made.

1B. "Stage 4". Sorry if you take umbrage at the phrase "careful" experimenters. It is very difficult to write clearly what I mean as it encompasses many aspects. Let me try and give you a few examples

a) The Harwell group were "careful" - they did many controls and had many cells and counters working simultaneously. I would strongly recommend you to read their paper in Nature of 23 November, vol. 342 (1989) 375. David Williams told me last week that they are nearing completion of their full paper - the Nature one is just a summary.

b) Extensive calibrations and controls are essential.

c) The most obvious and essential control is to repeat the experiment with normal water - if excess heat is still observed, then the cause cannot be deuterium fusion. Glad to see that you say you are now starting with H2O. An important question is what should be the ratio of running with D2O to H2O. If the effect seen with D2O is strong and reproducible, then maybe only 10% of the time is required, but if the effect is not reproducible, then about equal time should be given to H2O running. One has to look at the errors. Let me quote to you part of a letter of 31 Jan from a very good group leader who had been getting cold fusion effects; "We believed to see a clear pattern in anomalous emissions (of neutrons) because we had a huge amount of background data with no sign of anomalies. However, as often happens in the search for rare effects, it is rather difficult to attach a likelihood to the above statement and the recent measurement (had suggested to him repeating the measurements with some additional instrumentation which would allow him to check if there were a simultaneous other sign) seems to exclude anything electromagnetic coming from the deuterated titanium. With these last measurement, I consider closed my contribution to this field"

d) Another important control is to use D2O but with inert electrodes that do

not take up deuterium. And I see that you have done this by using Pt - Pt electrodes. You write that you observed " a net power of 86 mW before closing the cell and 5 +/- 20 mW after". This would seem to indicate an open cell gives excess heat but a closed cell does not (supporting my strong bias in favour of closed cells). Hope I have understood correctly - a tabulated summary of your experiments and results would clarify things.

If you are observing power with a Pd cathode and not with a Pt cathode under identical conditions, then this would be an indication of excess heat from a cause other than CO₂. What are the numbers?

2. Most experiments are not good first time. One learns by doing. As you say your next generation will be better. Having everything inside a constant temperature bath (or dewar) is safer.

3. can you correlate these peaks in power with any other effect?

4. Your saw tooth voltage wave form is interesting and the first report I have heard of such an effect. Bubbles do tend to grow in the same place when there is a slight irregularity in the surface, e.g. a spike (dendrite) will be a favoured spot because of surface tension effects. If the rods cracks as the hydrogen or deuterium enters and causes it to expand, this would naturally give irregularities.

Overall, one should continue as long as one enjoys it (Edward Teller told me this was his attitude to Cold Fusion - he does not believe in it but it is fun for him to try and theoretically explain some claims).

When I was in Berkeley last month was finally able to buy Luiz Alvarez's autobiography, "Adventures of a Physicist". Enjoyed it so much that am now reading it for a second time - think you would enjoy it too. There is one bit in it where he and a colleague spent two weeks to try and stop their boss Livingstone, from publishing his apparent discovery of a particle with the mass of a proton and negative charge (the antiproton had not been discovered and the accelerator being used was of too low energy to produce it). So Luie believed his boss must be wrong, but it was only by changing the experimental conditions they were able to prove to him that it was a hydrogen atom with a negative charge.

Best Wishes,

Douglas.

New Energy

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Received: from CERN by CERN.cern.ch (Mailer R2.05) with BSMTTP id 9054; Tue,
20 Feb 90 11:09:11 GVA
Received: from dxmint.cern.ch by CERN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Tue, 20 Feb 90 11:09:09 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA15707; Tue, 20 Feb 90 11:06:20 +0100
Message-Id: <9002201006.AA15707@dxmint.cern.ch>
Date: Tue, 20 Feb 90 11:11 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear Dick, 20 February 1990.
Received a letter from Tom Droege who does not seem to be too
familiar with scientific methods. He says he has written similiarly to you.
Sent him the letter below. Hope you can help him.
Best Wishes,
Douglas.

Dear Tom, 20 February 1990.
Received your surprising message. You are correct that when they
were observing excess heat they did not simultaneously observe AC. And their
paper as you say, is carefully written to say this.
However put yourself in their place. They are trying to do an experiment
to search for excess heat produced by fusion of deuterium as reported.
Therefore they set up to measure excess heat and fusion products. And like
good experimenters, they simultaneously have an identical control experiment
with normal water. For a long time nothing is observed in either cell. This
gives their first result - there is no simple effect. Remember Martin
Fleischmann told me that it was a simple table top experiment and there
was no secret.

After some time both their cells appear to give excess heat. But if it is
both their cells, the effect cannot be from fusion. This second result is
contrary to the Fleischmann and Pons basic result.

During the period of apparent observation of excess heat, they observed no
fusion products. This confirms their second result that there is no relation
between the apparent observation of excess heat and fusion.

Now as scientists they have to try and explain these apparently
contradictory results.

They note that "excess heat" is the difference between the apparent heat in
and the measured heat (or power) out. They realise that that they are only
measuring the DC power in and not any AC power. So they do a simulation and
find that when they add AC power, this is not recorded in their original
set-up, but excess heat is apparently observed in both the H2O and D2O, and no
fusion products are observed. That is they have simulated their results. Hence
they have a reasonable explanation of the "excess heat". And they have a result
that under normal conditions there is no excess heat and no fusion products.

This seems reasonable. Now maybe one might ask them to continue running with
devices to measure both AC and DC currents. But since everyone agrees that any
"excess heat" observed is erratic, how long would they have to run for since
they now know that oscillations should be avoided. Should they run or should
they return to their normal work? How do you reproduce an irreproducible
result?

There are many books about the scientific method - the ones I like best are

by Medawar.

Most scientist I work with play at fun ball and true ball but not hard ball.

Hope your meeeting with Dick goes well,

Best Wishes,

Douglas.

New Energy Times Archive



26

Dr. Richard L. Garwin
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY
10598

POSTAGE DUE 20¢

Date: 18 February 1990, 12:57:30 EST
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVNV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: JONESSE at BYUVAX
cc: GAI at YALEVM
Subject: Your letter to me of 02/02/90.
Reply-To: RLG2 at WATSON

Chad Jones
Box 218

I had heard about your letter to me and wondered when I might see it.

The mystery was resolved when it arrived 02/15 with a 25-cent stamp and 20-cents postage due. Why the post office delays postage-due mail I don't know, but they do.

In any case, I read the letter with interest and believe that it is just this sort of question that must be resolved in determining the significance of experiments, whether at Yale or at LANL or at BYU.

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JONESSE NOTEBOOK AO V 132 Trunc=132 Size=178 Line=178 Col=1 Alt=0

X E D I T 1 File
IDLE

this sort of question that must be resolved in determining the significance of experiments, whether at Yale or at LANL or at BYU.

Certainly it is important to publish DETAILS of experiments, and just as important to publish negative experiments that are carefully done as it is to publish details of experiments that report neutrons or tritium or heat.

Best regards,

Dick Garwin
* * * End of File * * *

New Energy Times Archive

BRIGHAM YOUNG
UNIVERSITY

DEPARTMENT OF PHYSICS AND ASTRONOMY
296 EYRING SCIENCE CENTER
PROVO, UTAH 84602

Feb. 2, 1990

Dr. Richard L. Garwin
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598

*Sent with 25¢ stamp.
arrived with 20¢ "patgo due"*

Dear Dr. Garwin,

Thank you for your memo to me (subject: Conclusions) which I received Jan. 29. I agree that your calculations with hit probabilities agree very well with Dr. Anderson's Monte Carlo calculations, and that "the paper should quote some of the Monte Carlo results of Anderson" as you suggest.

I would like to respond to the important comment you make about the number of bursts expected in the Yale experiment:

"...one can simply say that no neutron bursts exceeding 40 occurred within the apparatus at Yale during the running time of the experiment. And one could go on to say that if the conditions were the same as at LANL, some xx events exceeding $M=40$ would have been expected."

I would like to explore the number "xx" of expected bursts; it is easy to show that this number is less than one.

First, let us note that the premise of "no neutron bursts exceeding 40" depends on the assumption that (ring) detector efficiencies are 0.8%. Anderson's analysis shows that the efficiencies vary among the detectors, and that the average efficiency is probably closer to 0.5%. (For instance, when 2-dimensional (pulse-shape/pulse-height) cuts are applied to the data, the acceptance of neutrons is clearly less than 100%; see figure 1 attached. Al also identifies non-negligible electronic inefficiencies.) The premise also depends on NO signal of 2 or more votes in ring counters; Anderson's analysis indicates otherwise (see fig. 1). So the premise is very generous to Yale as we compare with Los Alamos.

Table II from the Menlove, Garcia, Jones paper (LA UR 89-3633) shows 20 bursts having over 40 neutrons (you found, correctly, 19 bursts of 50 or more neutrons), if we count bursts during the warm-up period, i.e., while the sample temperature is between -100 C and 0 C.

I will use your suggestion that we compare burst rates just during the warmup period. However, we find that the hydriding proceeds very slowly in our Ti 6-6-2 samples (see figure 2) since they were annealed below 400 C. Thus, the "soaking time" at room temperature probably should not be neglected. Again, to be generous to Yale, let's use just the warm-up time, since at LANL this time is 4-5% of the total, while about 20% of the experimental time at Yale -- figure 3.) The relevant times are approximately 20 foreground hours at Yale (fig. 3) compared with 600 hours at Los Alamos.

020290.SFJ

(600 hours out of approximately 13,000 total FOREGROUND data cylinder-hours at LANL over four detector systems, at ~ 100g sample per cylinder, as determined by E. Garcia and confirmed by H. Menlove. Note that sometimes more than one cylinder was used in a detector at a time, but that the hours apply to each cylinder (cylinder-hours). These numbers are relevant to the Menlove, Garcia, Jones paper LA-UR 89-3633 which was distributed to the collaboration in draft form in October 1989 along with my first estimates of "xx". Table II (attached) provides essential data. This paper represents about 6 months of essentially continuous data-taking at Los Alamos. Incidentally, bursts were not uncommonly seen after a dozen temperature cycles in the Ti 6-6-2 material, a new observation since the original Menlove submission to *Nature*.)

We need to adjust for the fact that the sample cylinders were larger at LANL, holding about 100g sample each, whereas at Yale up to four cylinders were used, holding a total of about 200g.

Finally, the time gate over which the systems were sensitive to neutron detection differed greatly between Yale and LANL detectors. The LANL detectors were active for 128 μ s, whereas the time gate at Yale was only 20 μ s (actually \pm 10 μ s after the first neutron detected in a central counter. Incidentally, the two central counters at Yale were not timed together.) Not knowing the time structure of neutron bursts, we scale by the relative event gate widths: 20 μ s (Yale) / 128 μ s (LANL).

Putting these factors together and recalling the effort to be generous to what we can expect from the Yale set-up, I estimate the number xx of bursts of > 40 neutrons expected at Yale:

$$\begin{aligned}
 xx &= [20 \text{ bursts of over } 40 \text{ neutrons each at LANL}] \\
 &\quad \times 20 \text{ warm-up hours at Yale} / 600 \text{ warm-up hours at LANL} \\
 &\quad \times (200\text{g/hr at Yale}) / (100\text{g/hr at LANL}) \\
 &\quad \times 20 \mu\text{s event gate at Yale} / 128 \mu\text{s event gate at LANL} \\
 &= 0.2 \text{ bursts expected at Yale.}
 \end{aligned}$$

The probability of detecting a burst with 0.2 events anticipated is $1 - \exp(-0.2) = 0.2$. If we assume (generously) that the short time gate at Yale does not affect the burst detection efficiency, we find a probability of $1 - \exp(-1.3) = 0.7$ of detecting a burst.

More realistically, take the ring counter efficiency (average) to be 0.5% (see Anderson analysis: same value as in original Yale/BNL paper in *Nature*). Then 2 or more ring-counter hits

requires a source of 90 neutrons in a burst for a 93% probability of detection (figures 4, 5 herewith). There are thirteen bursts at LANL during warm-up of 90+ neutrons (Table II attached), so:

xx (realistic) = 13 bursts at LANL X 20hrs/600hrs X 200g/100g
X 20 μ s/128 μ s
= 0.14 bursts expected at Yale
= 0.87 if time gate is ignored (58% probability of detecting 1 burst).

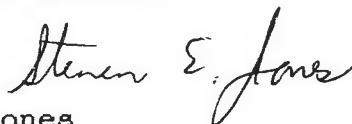
I conclude that the Yale results (even assuming a null result, but see Figure 1), are not in disagreement with the Los Alamos results.

I should comment briefly also on the likelihood of observing random neutron production in the Yale experiment. (Anderson discusses this in his report on the data, pp. 1-3 and Appendix A.) Menlove's submission to *Nature* discusses three several-hour episodes of random neutron emission from D2-gas experiments, with one episode occurring on average every 600 cylinder-hours (approx.) of foreground data. (Ed Garcia, private communication). There is no correlation noted here with temperature cycling. The comparison with Yale is straightforward: one would expect on average one episode of random neutron production at Yale every 300 hours (scaling by the sample masses, 200g at Yale/100g at LANL). Since the total foreground running time at Yale was 103 hours, 0.3 episodes are anticipated. Thus, I estimate a probability of seeing random neutron production at Yale, assuming conditions like those at LANL, of only about 25%. A null result (at roughly 75% probability) does not contradict Menlove.

I would suppose that the journal *Nature* would like a paper to go along with the Menlove paper submitted over eight months ago. The Sandia experiment is not of sufficient sensitivity to challenge the Menlove results (M. Butler, communication to a group assembled in Germantown, MD, Dec. 6, 1989). If the truth be known, the Yale experiment will not do the job either.

An experiment with higher sensitivity is needed to challenge the LANL results, and that is what we are now organizing at Los Alamos, building on the muon-catalyzed fusion detector system (see PRL 51, 1757 (1983), 56,588 (1986), *Nature* 321,127 (1986)). I have invited the previous collaborators to join (and not impede) this effort.

Sincerely,



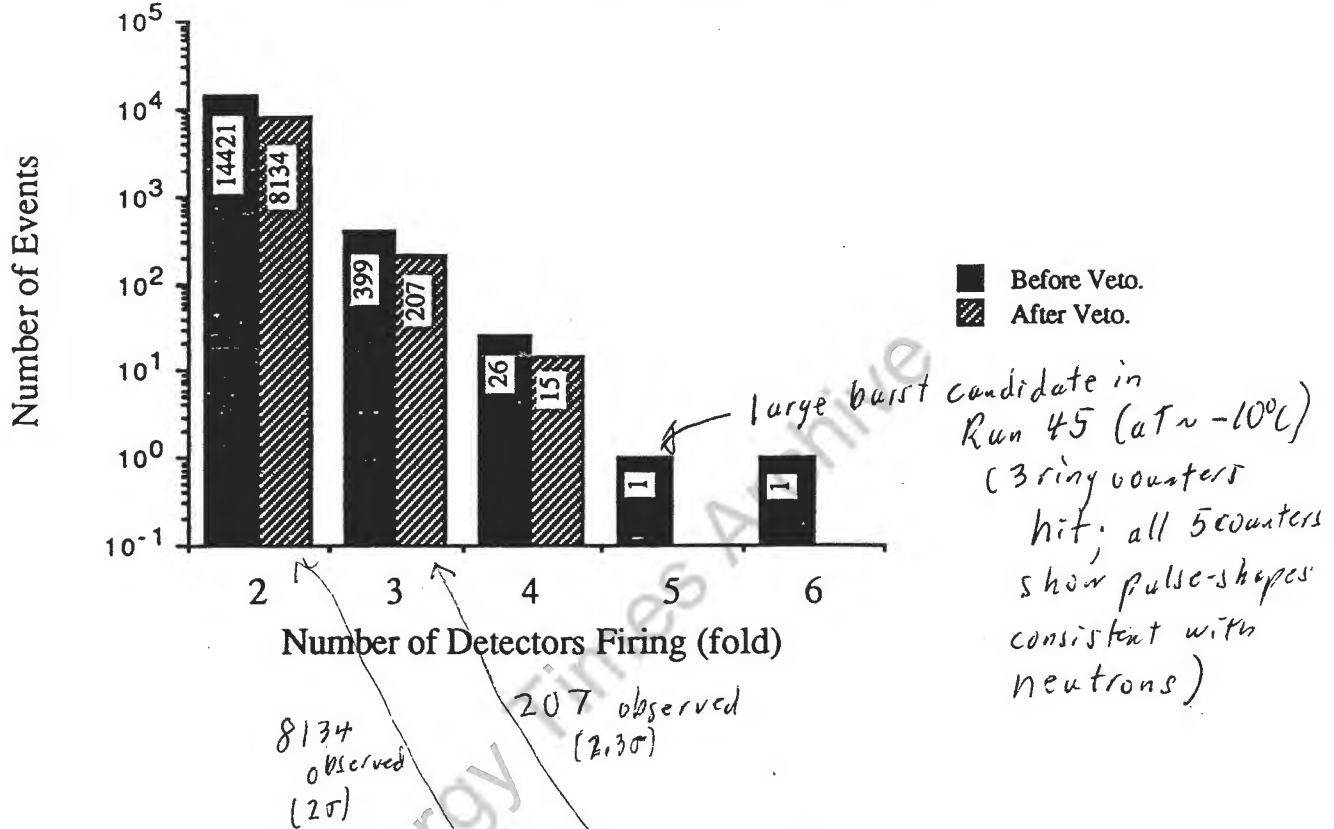
Steven E. Jones

cc: J. Huizenga, J.P. Schiffer, D. Morrison, G. Taubes, D. Lindley (Prof. Gai's CC list, memo of 26 Jan. 1990)

cc: M. Gai, K. Lynn, K. Zilm, A. Anderson, S. Koonin

Fig. 1 - From "Interim Report" (Oct. 1989)
 After pulse-shape and pulse-height cuts to select clean neutron events, Dr. Al Anderson finds 14 foreground events, 3 background events of 2 ring-counters hit. (Fore-Back) Rate is $(0.15 \pm 0.09)/hr$, in dataset.

High Fold Events From Data Runs (103 Hrs)



High Fold Events From Background Runs (28 Hrs)

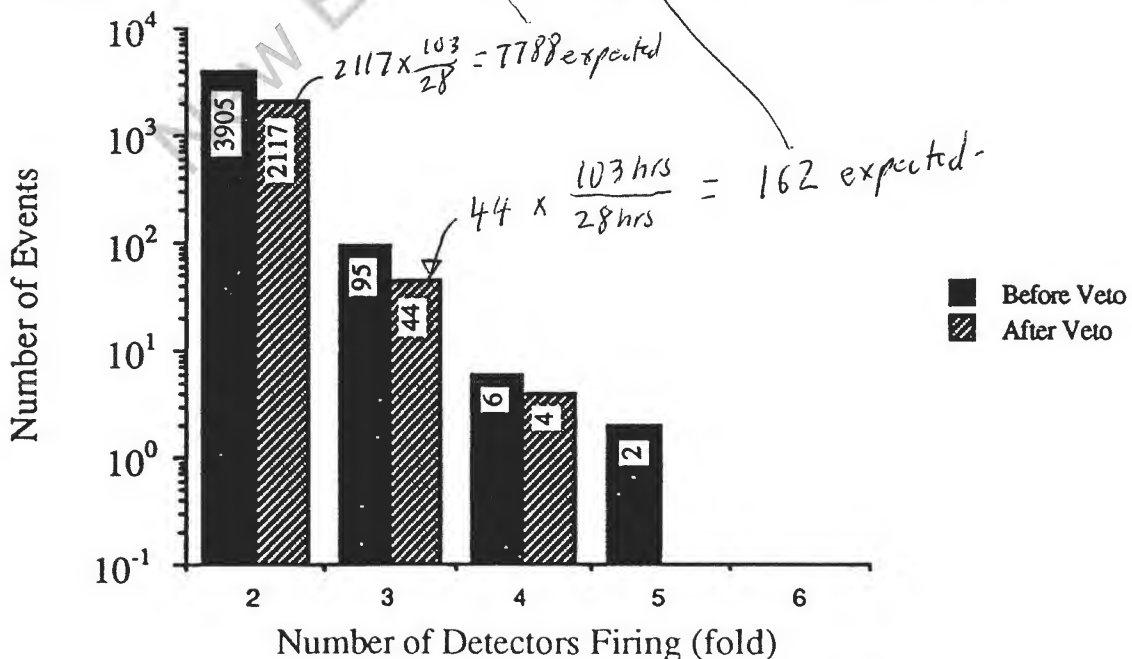


Fig. 3

Are NO burst events seen??

624

The minimum annealing temperature required for a rapid hydriding of titanium sponge was determined by varying the annealing temperature and reacting all samples at 25°C. The results are shown in Fig. 3. The data shows that the minimum annealing temperature for titanium sponge is about 400°C; if annealed at 300°C the subsequent reaction at 25°C is very slow. Annealing temperatures above 400°C have little effect on the rate of hydriding of these sponge samples. A similar study was not done for zirconium sponge but the hydriding rates at 25°C were similar for samples vacuum annealed at 500°C and at 1000°C. Temperatures between 400-500°C seem to be sufficient to dissolve the surface oxides during the vacuum annealing procedure (Dushman, 1949) and allow rapid hydriding of the sponge initially at 25°C.

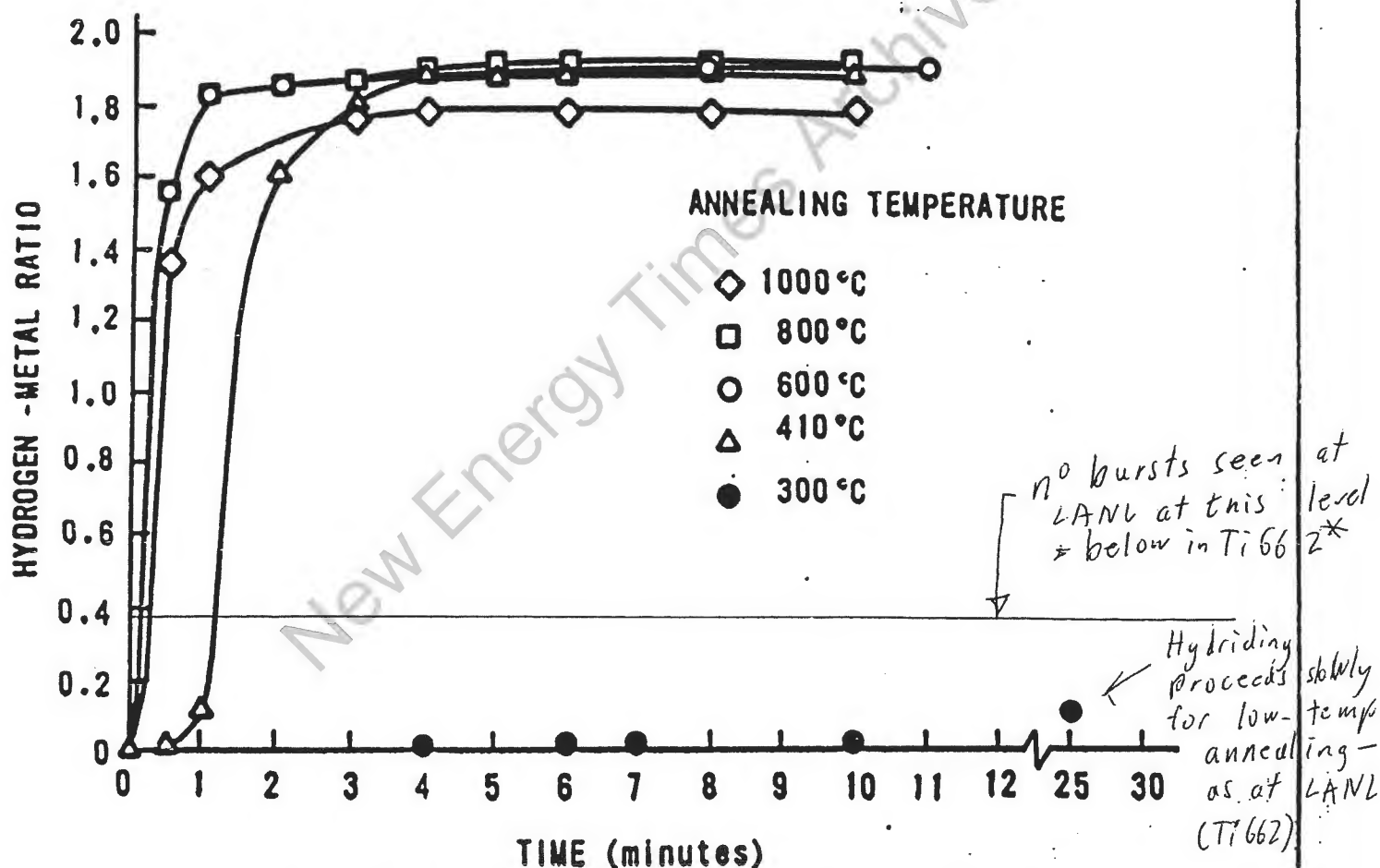


Fig. 3. Formation of titanium hydride from titanium sponge at an initial reaction temperature of 25°C after vacuum annealing samples at various temperatures.

* d/Ti ratio on initial loading;
local d/Ti (e.g. near surface)
could be much higher

From "Interim Report" (Oct. 1989)
 Total warm-up hours: 15 cycles (foreground) X 80 min/cycle = 20 hours

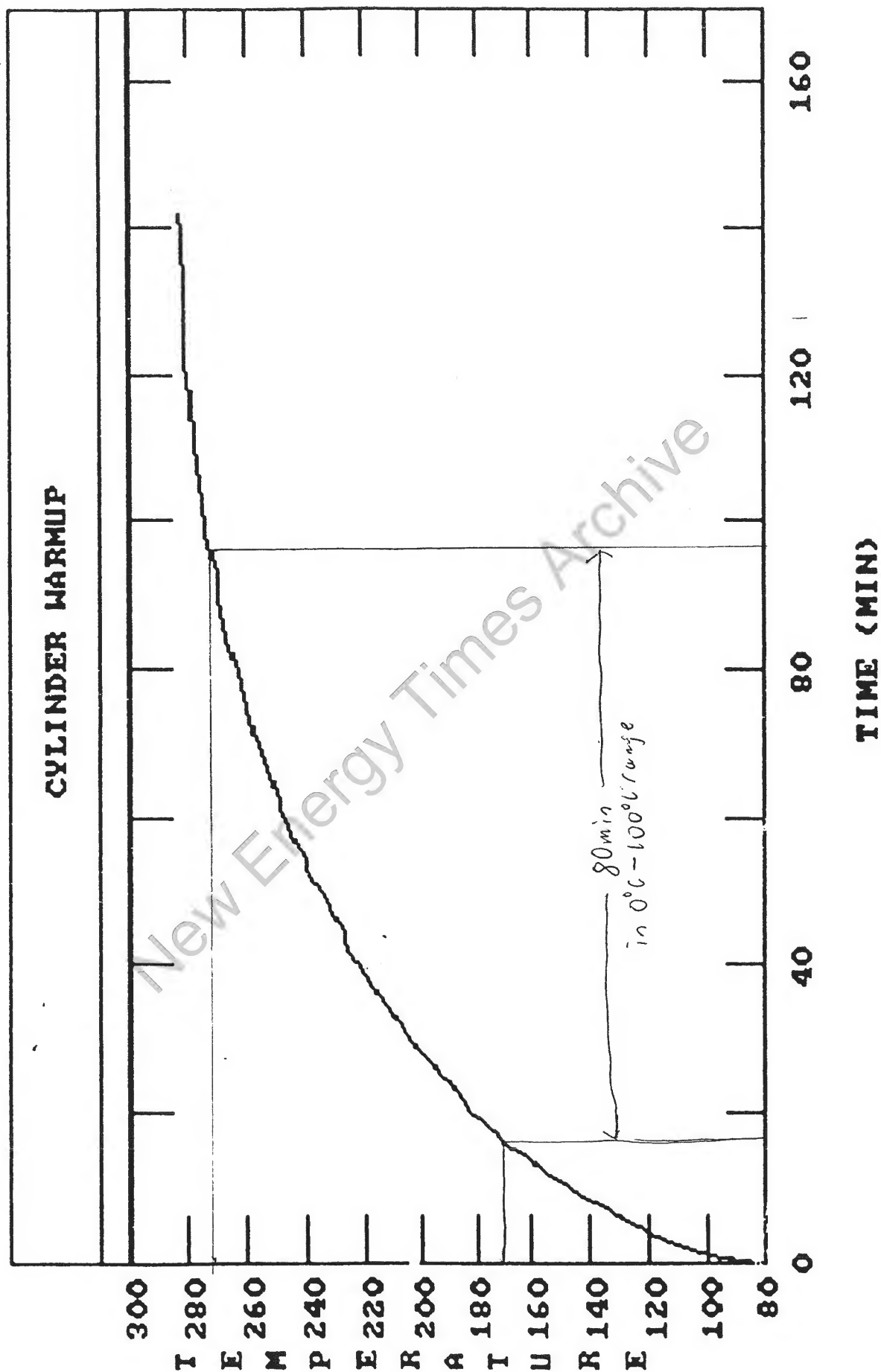
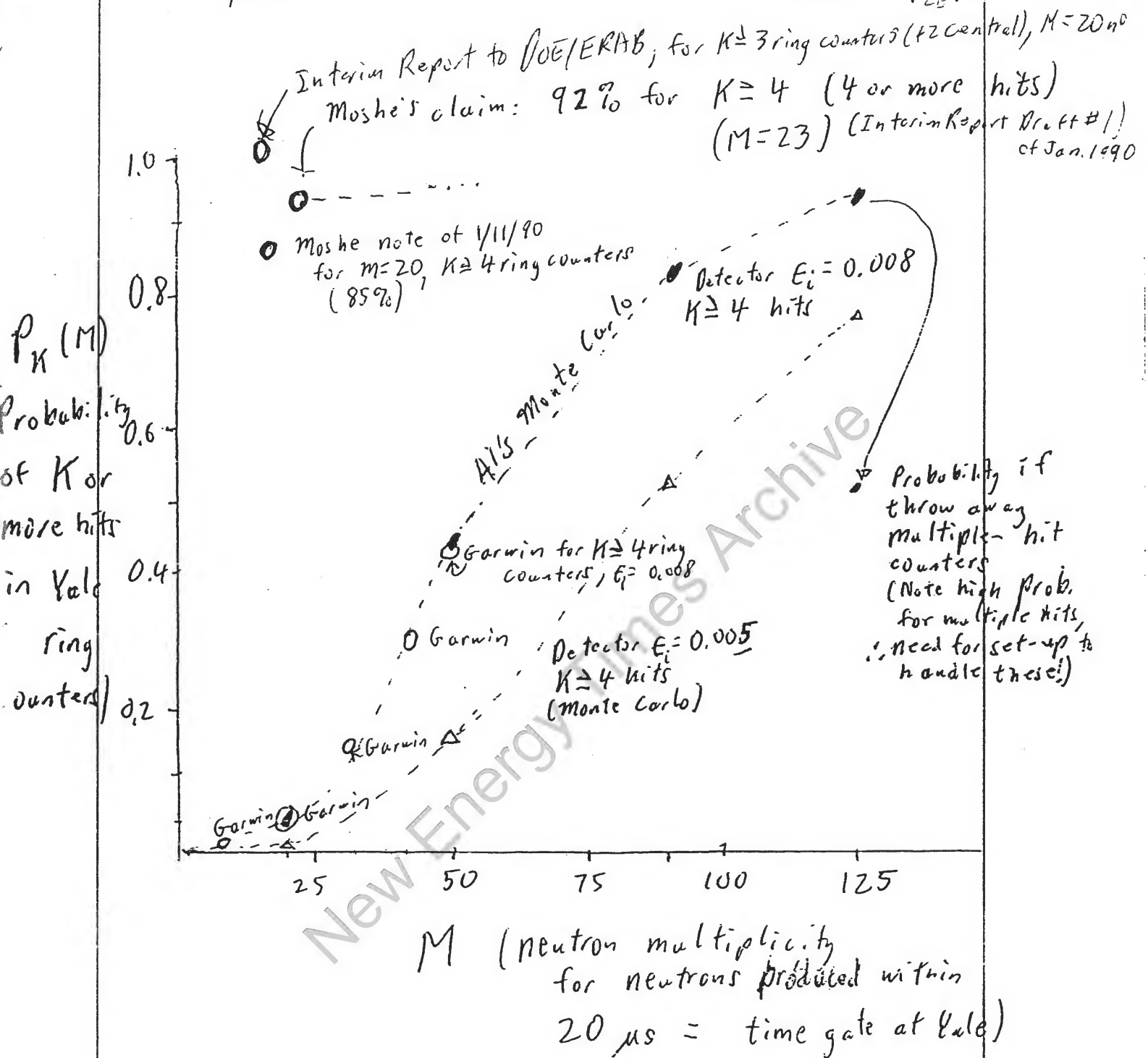


Fig. 3

FIG 4.

Jones 1/14/90



Probability of 4 or more hits in Yale ring counters; contrasting Moshe's calculations and Anderson's Monte Carlo calculations. Note that if the Monte Carlo is accurate, then the Yale experiment was not set up well to detect bursts.

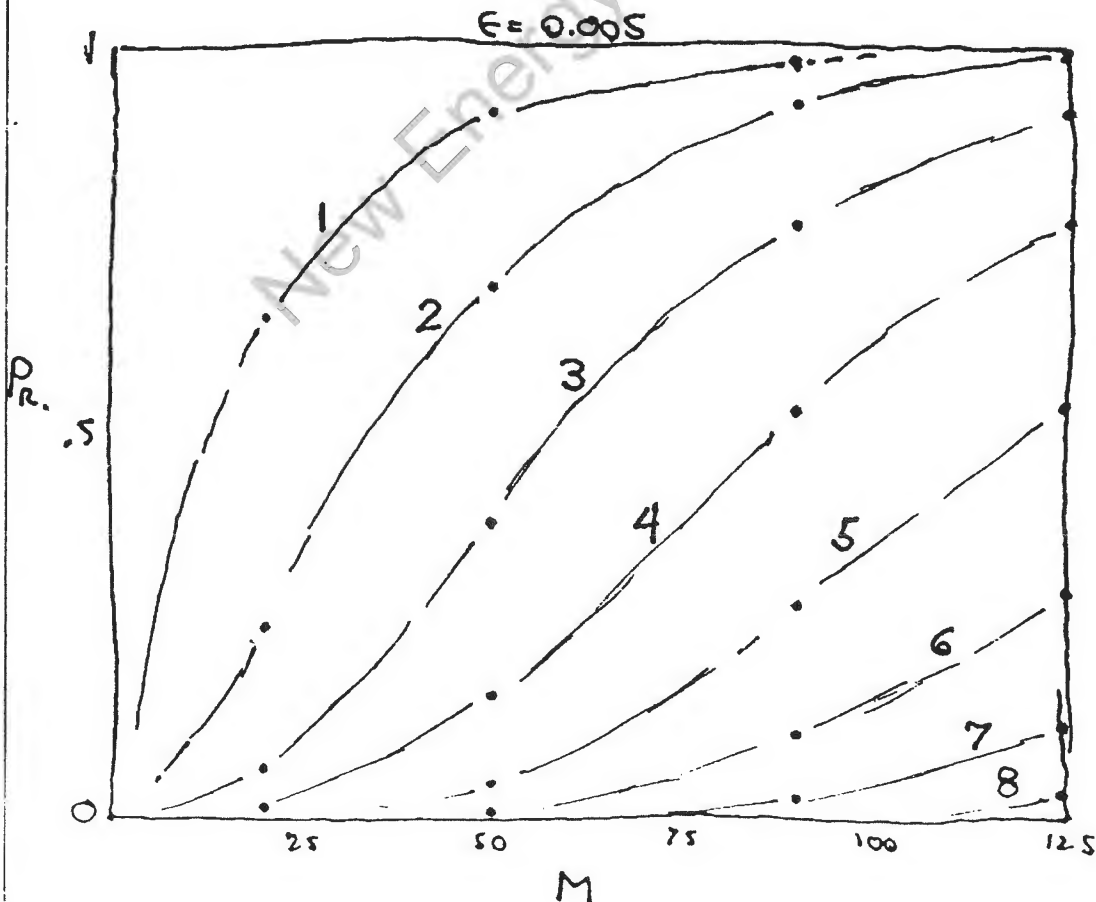
Fig 5.

(Alan Anderson)

Det effy 0.005 (10000 EVUTS) Rings ONLY (CERN 16AB27)

M	20	50	90	125
K hits (or more)				
1 or more	65.1	92.2	98.9	99.9
2	25.0	69.2	93.4	98.2
3	6.0	38.6	77.3	91.8
4	0.9	15.8	52.9	77.5
5	0.1	4.4	28.0	53.9
6	0	1.0	10.7	29.4
7	0	0.2	2.8	11.9
8	0	0	0.6	3.3
9	0	0	0.1	0.5
10	0	0	0	0

% Probability



From Menlove, Garcia & Jones, LA-UR-89-2655
 "Update on Measurement of n^0 Emission
 from Ti samples in Pressurized D_2 Gas"

(Draft sent to collaboration in Oct, 1989)

TABLE II. Active Samples--Burst Results

Starting Date (Mo/d)	Sample ^a	Number of Source Neutrons ^b
4/28	Ti-1	<u>127</u> , <u>136</u> , <u>85</u> , <u>76</u>
5/6	Ti-6	<u>258</u> , <u>179</u> , <u>121</u> , <u>70</u> , <u>15</u> , <u>127</u> , <u>94</u>
5/16	(Ti-10)	<u>12</u> , <u>15</u>
5/19	DH-1	55, 15, 15, 27
6/2	DD-2	12, 85, <u>15</u> , <u>15</u> , <u>39</u> , <u>142</u> , 18, 30
6/9	(DH-4)	21
6/12	(Ti-13)	24
6/20	Ti-14	15, 91, <u>12</u> , 24
6/28	Ti-16	24, 24, 12, <u>15</u> , 18, 15, 15, <u>109</u> , <u>18</u>
7/8	DD-56	<u>12</u> , <u>279</u> , <u>130</u> , <u>88</u> , <u>48</u>
7/18	Ti-19	88, 15
8/18	Ti-22	55, <u>97</u>
8/11	(Ti-23)	19, 23, 19
8/8	Ti-24	35, <u>119</u> , <u>16</u> , 16, 16, 15, 15
8/22	(Ti-25)	32
8/24	(Ti-28)	17
8/29	Ti-30	<u>280</u> , <u>13</u>
9/11	Ti-31A	<u>86</u> , <u>34</u>
9/12	Ti-32	<u>23</u> , 35

^aThe parenthesis around a sample number indicates a marginal performer that is on the borderline between active and inactive samples.

^bThe underline under the neutron yield indicates that the burst occurred during warm-up from LN temperature.

Most of the samples from Ti-12 to Ti-36 have used variations of the alloy Ti-6, 6, 2 (6% Al, 6% V, 2% Sn) or Ti-6, 4 (6% Al, 4% V). The metal bar stock is cut into small turnings (~1 by 1 by 3 mm) on a lathe.

An example of an active sample data run is shown in Fig. 2, where the coincidence counts per 2000-s time interval are plotted vs the counting time. The large emission at about 195 h occurred at the -30° temperature after removing the sample from the LN following a D₂ gas refill.

-R.L. GARWIN-
15 FEB 90 66

Fax: 203 432 3522

July 17, 1989

Dear Moshe -

In preparation for our "cold fusion" experiments, I'm sending information regarding preparation of the D_2 -pressure vessels and titanium alloy. I will bring the Ti alloy and pressure cylinders with valves and gauges. Can you then provide the D_2 + needed valves, small $\sim 200^\circ C$ oven, and vacuum gear?

Looking forward to working with you - we need to establish a time-frame of ~ 5 days or so for the experiments - perhaps the last half of August (so Howard Menlove could come).

Best Regards -

W.E. Jones

Jones requested
 ~5 days. We can
 complain that it
 was too short...

J.G.

YALE UNIVERSITY
 A. W. WRIGHT NUCLEAR STRUCTURE LABORATORY
 P.O. Box 6666
 272 Whitney Avenue, New Haven, Connecticut 06511

021590..MG

Dick

Prof. Moshe Gai -

FAX: 203 432 3522

Dear Moshe:

1- The specifications for T: 6-6-2 alloy

were sent (express) Dick

with a sample

He should have in

pre-print was also

2- One straight forward

problem is to increase it at 20 μ sec

5 μ s. This requires

modification in the TV

at 2 AMPF, I can,

3- How can we d

from a single neu

by random neurons (B)

Here Jones finally

agrees to increasing

the gate width

to 5 μ sec. I set

it at 20 μ sec

during the experiments

(in spite of Jones

objection) Now he

says the gate was

too

short. H.G.

B $\sqrt{\frac{1}{2}}$ det. 0
source

These can be separated statistically - depending
on time-structure and multiplicity of no-burst
events. Have you thought about a solution?

15 FEB 90 9.66

-R.L. GARWIN-


Prof. Moshe Gai -

FAX: 203 432 3522

Dear Moshe:

1- The specifications for T: 6-6-2 alloy were sent (express) last Friday, along with a sample of T: 662, to John Hark. He should have it by now. The Menlove pre-print was also sent.

2- One straightforward solution to the TDC problem is to increase the time interval to 5 μ s. This requires a straightforward electronics modification in the TDC which is done routinely at LAMPF. I can get details if you need them.

3- How can we distinguish multiple hits from a single neutron (A) from multiple hits by random neutrons (B)? e.g. 

These can be separated statistically - depending on time-structure and multiplicity of neutron events. Have you thought about a solution?

Received: by YALEVM (Mailer R2.03B) id 3739; Thu, 08 Feb 90 11:50:05 EST
Date: Thu, 08 Feb 90 10:49:05 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Estimate of number of bursts
To: Steve Jones <jonesse@byuvax>,
"R.L. Garwin" <rlg2@watson>,
Kelvin Lynn <kgl@apsedoff>,
John Schiffer <schiffer@anlphy>,
John Huizenga <huizenga@uorchem>,
Steve Koonin <koonin@caltech>

Dear Steve,

Your letter to Dick Garwin of Feb 2nd. includes one oversight and one assumption:

1. While for the LANL-BYU data you count cylinder*hours (600), for the Yale data you count real hours (20). The number for the Yale data is in fact 80, as we outline in our paper that will be submitted shortly.

All above running times are while the cylinders are warming up. You have arrived at the estimate of 600 hours for LANL-BYU, by taking 5% of 13,000 hours or so. This period of 13,000 hours or so, corresponds to 541 days, or 1.5 years. Clearly the time in LANL-BYU estimate is not real time, but normalized time. The time you took for Yale is real time! You then need to multiply the Yale run time by the factor of 4, for the 4 cylinders.

When correcting this factor, and not including the shorter duration of the gate used at Yale, you in fact predict 4 bursts for the Yale experiment.

2. You have added a new twist to the calculation which is the gate duration, admittedly a factor of 5-6 smaller in the Yale experiment. In this you make the assumption that the burst is in fact spread over the entire observation time of LANL-BYU. If this is correct then this factor should be included.

In this context I should remind you that on July 11th, while preparing the Yale experiment you have suggested the gate duration of 5 microsecond only. In your letter you made the plausible argument that you expect the bursts to be simultaneous. It was only due to my suggestion that the gate width was increased to 20 microsecond. It was only due to your insistence, in writing, that the gate duration was not made longer, since you wanted to study the time structure of bursts with higher accuracy.

Your note also includes another less important mistake. The two central detectors at Yale were in fact timed. I am surprised that as a person who took part in the data taking you will write: "Incidentally, the two central counters at Yale were not timed together." This is simply factually wrong, and I thought we discussed it already and clarified this point.

Concerning your proposal for an experiment at LAMPF. As I outlined to you, your proposal is in fact flawed, see my letter of Feb. 5th. I feel you are going to do another one of these inconclusive experiments as you did with Menlove et al.

If you are really serious about performing a conclusive test, then I propose you accept the invitation of the BUGEY collaboration to do an experiment at the FRESUJ Tunnel in the French Alps. You are reminded that Yves Declais invited you to do this experiment when you were at CERN and during the Santa Fe

meeting.

The BUGGY collaboration have a background of 2 neutrons in 5 days. Every neutron you will see there is a convincing one. In addition they have 98 neutron detectors, which are doped with ^6Li , and therefore act as spectrometers. As I outlined to you in my letter of Feb. 5th. the correct way to search for bursts is by having small efficiency per detector (to reduce multiple hits) and increasing the number of detectors. Your LAMPF solution, of using 4 detectors (instead of the 12 used at Yale) is going in the wrong direction. The BUGGY collaboration is in fact providing you the correct solution, in a correct low background environment. Before you go to BUGGY I can assure you that no reasonable physicist will believe that you are in fact sincere about testing if "cold fusion" really exist.

I am looking forward for receiving your comments on draft 3, which was FED. EXP. to you on last Tuesday.

Regards Moshe Gai.

New Energy Times Archive

Received: by YALEVM (Mailer R2.03B) id 4559; Thu, 08 Feb 90 13:05:24 EST
Date: Thu, 08 Feb 90 12:51:47 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Copy of notes from Jones to Gai.
To: Dick Garwin <rlg2@watson>,
Steve Jones <jonesse@byuvax>,
Kelvin Lynn <kgl@apsedoff>,
John Schiffer <schiffer@anlphy>,
John Huizenga <huizenga@uorchem>,
Steve Koonin <koonin@caltech>

Dear Dick,

For the record:

I mail to a copy of a note from Jones to me dated July 17th, 1990.
There you will find: "We need to establish a time-frame of 5 days or so for the experiment - perhaps the last half of August (so Howard Menlove could come)." The Yale experiment run for 10 days August 20-30. Jones now tell me we run for too short of a time.

In the note dated August 8th. you find: "One straight forward solution to the TDC problem is to increase the time interval to 5 microsecond." In fact at the Yale experiment individual TACs (not many channel TDCs) were used, with a flexible time range. We used 20 microsecond gate (instead of the 5 Jones suggested) and now I am told, that we should have run 128 microsecond gate.

Off course, this discussion can go on forever, I can make such kind of arguments on any experiment. I am very eager to get our data published as it is, and will be very thankfull if you comment on our draft number 3, that was FED. EXP. to you.

Best Regards Moshe Gai.

Received: by YALEVM (Mailer R2.03B) id 6227; Sun, 18 Feb 90 12:44:40 EST
Date: Sun, 18 Feb 90 12:28:46 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Research of "cold fusion" at Paris.
To: Steve Jones <jonesse@byuvax>,
Kelvin Lynn <kg1@apsedoff>,
John Huizenga <huizenga@uorchem>,
Dick Garwin <rlg2@watson>,
John Schiffer <schiffer@anlphy>,
Steve Koonin <koonin@caltech>,
Peter Parker <pparker@yalevm>

Dear Steve,

I forward to you a few publications of: "Five laboratories of the University P. et M. Curie and Paris VII have carried out in collaboration, a series of experiments on cold fusion". (Received from Professor Huizenga).

"The conclusions of these experiments, presented in the ecnclosed papers, are the following".

(i) "In the Jones experimental conditions of electrolysis, deuterium does not appreciably penetrate into the titanium foils ... all cations in the Jones solution (K, Fe, Ni...) are first deposited on the foils, blocking any further deuterium penetration..."

(ii) "Deuterium has been efficiently accumulated in titanium foils by gas diffusion at high temperature or by electrolysis of solutions containing F ions ... So far no fusion event has been recorded".

You will note that the conclusion that deuterium did not penetrate the Ti foils is identical to that of the Yale experiment.

I am frustrated that a year after the announcement of "cold fusion" you have still neglected to quantitatively characterize the deuterium loading in your electrodes, in spite of the fact that we have expressed this concern to you on several occasions. This was now done in the Paris work.

With best regards Moshe Gai.

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Date: 18 February 1990, 12:57:30 EST
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: JONESSE at BYUVAX
cc: GAI at YALEVM
Subject: Your letter to me of 02/02/90.
Reply-To: RLG2 at YKTVMV
Reply-Bit: RLG2 at WATSON
Reply-Xin: rlg2@ibm.com

I had heard about your letter to me and wondered when I might see it.

The mystery was resolved when it arrived 02/15 with a 25-cent stamp and 20-cents postage due. Why the post office delays postage-due mail I don't know, but they do.

In any case, I read the letter with interest and believe that it is just this sort of question that must be resolved in determining the significance of experiments, whether at Yale or at LANL or at BYU.

Certainly it is important to publish DETAILS of experiments, and just as important to publish negative experiments that are carefully done as it is to publish details of experiments that report neutrons or tritium or heat.

Best regards,

Dick Garwin